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A Sense of History in Science*

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THE words "history" and "science" are often thought to be connotative of extremely different, if not mutually exclusive, disciplines. History is the study of events that occurred in the past while science, a cumulative discipline, embodies the discoveries of the past insofar as they are valid or relevant in the light of our present knowledge—but without reference to the conditions under which they were made. For the practicing scientist it is important to know that the ratio e/m has the value 5.27×10^{17} esu/gram and that this value may be obtained in a highly evacuated instrument with deflecting plates, etc., when the applied voltage is sufficiently low to produce electrons whose mass does not require a relativistic correction; the scientist need not know today that J. J. Thomson did his research before the introduction of our modern high vacuum techniques, nor the ways in which the relatively poor vacuum hampered J. J. Thomson's research. But the historian of science is concerned with just such problems; he wants to know just how J. J. Thomson experimented, what the effect actually was of using poor vacuum, why J. J. Thomson was led to make these experiments, how he came to his conclusions that the atom was no longer to be considered the primary, indivisible particle of matter, what was the immediate reaction to these conclusions, and what was their long-term influence, and a host of other things. That the physicist—and often the teacher of physics—cares little for the aspects of history may readily be seen in the fact that the experiment whereby e/m is determined, as found in a great many textbooks of physics and described as "J. J. Thomson's," is described as if performed in a highly evacuated tube, usually with a heated cathode. Here, if a potential difference V is applied between the heated cathode and anode, cathode rays of speed v are produced, where

$$Ve=\tfrac{1}{2}mv^2.$$

Clearly, this is a simpler experiment to explain to beginning students than the low vacuum experiment J. J. Thomson actually performed. Thomson himself wrote about his experiments:

"It is not possible to estimate from the potential difference between the cathode and anode of the discharge tube the energy possessed by a charged particle at any point in its course without knowing more about the mechanism of the discharge than we do even at the present time. It is not even possible to determine the limits between which this energy must lie. Thus, for example, a charged particle starting from the cathode would ionise the gas through which it passed and produce other charged particles. These again would produce other charged particles when energied by the electric field, and these again would produce other charged particles. These secondary particles would not acquire as much energy as they

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would have done if they had started from the cathode itself. [etc.]"[1]

I have purposely chosen to begin this article with an example from fairly recent physics so that the discussion will not, at the outset at least, be subject to the criticism of "antiquarianism." But I believe that this example may serve to focus our attention on the question of whether what we tell our students is true or false. It would seem to me to be an elementary postulate of truthfulness that if we wish to describe something called "J. J. Thomson's experiment," we should describe the experiment that J. J. Thomson actually made; if we find it expedient to present to our students a modified form of the original experiment, why not say so? If this is true of the physics of the last sixty years, how much more true it is of the physics of three hundred years ago!

Let us examine what the history of science comprises, how nonhistorians may learn how best to use historical materials, and also see what pitfalls await the teacher of physics who is anxious to include some of the material from the history of science in his elementary courses. There are, at present, a sufficiently large number of elementary courses in physics which include historical information to justify a discussion of this general topic in the journal published by the American Association of Physics Teachers.

When I was still a graduate student working for my Ph.D. in the history of science, I used to be asked: "Are you an historian or a scientist?" In the pride of youth, I used to answer: "Both." Now that I am no longer a graduate student, and have more humility, I am more apt to answer: "Neither." For the historian of science is more than a hybrid in the academic garden, something more than a cross between two disciplines. Like the historian of ideas, of literature, or of political theory, his essential outlook, point of view, method of analyzing source material, and criteria of evaluation, are those of the trained historian. The Committee on Higher Degrees in the History of Science at Harvard University has wisely recognized that the historian of science is primarily an historian and they have therefore insisted that each candidate for the degree exhibit training and competence in one or more ordinary historical fields as well as in historiography. The historian of science differs from his brother historians in that the field to which he applies his historical training is the development of science, rather than the more traditional fields; hence, the historian of science has the added burden of knowing something about the science whose development he wishes to study. The more recent the period he wishes to study, the more advanced his knowledge must be; for example, to study Babylonian mathematics does not require a knowledge of topology or projective geometry. Doctoral candidates for the degree must have done graduate work (normally including experimental as well as theoretical study) in at least one scientific field, and have a sound elementary knowledge of a second scientific field.[2]

I would like to stress these dual qualifications —scientific and historical—because they help us to understand why there are so few professional historians of science. Most of the courses offered in our colleges in the history of science are taught either by historians or philosophers, on the one hand, or by scientists, on the other. So few of our colleges and universities offer full-time employment in the history of science (only three in America, to my knowledge), that most historians of science must find gainful employment teaching other subjects. Thus, to take but one example, while America has produced two outstanding historians of astronomy, neither one has ever offered instruction in the history of science, much less the history of astronomy.[3] The relevance of this discussion to the main topic in hand is that the history of science is an extremely young discipline with few sincere, professional adherents; as a result, most of the writings in any field of the

[1] Quoted from J. J. Thomson, *Recollections and reflections* (G. Bell and Sons, Ltd., London, 1936), pp. 339–340.

[2] Often there is an association made between the history of science and the philosophy of science, as in "Section *L*" of the AAAS. All too often, the writings in the philosophy of science—perforce containing some historical material—are confused with writings in the history of science. While many good historical works are written by philosophers of science, the latter, as a group, tend to use historical material for a specific purpose (i.e., as an illustration of a particular philosophic point) rather than to study an historical issue in full. Hence, the philosopher of science is more closely akin to the teacher of physics (both using the history of science for illustrative or heuristic purposes) than to the historian of science proper.

[3] One teaches English literature in a Western university and has not even had a doctoral candidate who wrote his dissertation on a scientific topic; the other teaches political history in an Eastern college.

history of science are produced by amateurs—many of them, to be sure, gifted—and the standards of scholarship are far lower than, say, in American history or ancient history, or any branch of science. Furthermore, most teachers of physics will not encounter on their local faculties a trained historian of science who can advise them as to reading or who can answer questions. Hence, some kind of guide in the history of science is badly needed by teachers of science who would like to make better use of the history of science than they do at present. In the following pages, I will attempt to erect a few guideposts of this kind.

In what follows, my remarks are based on a "maximum" goal, that of the physics teacher who wishes to gain a true insight into, and mastery of, the history of his subject—a time-consuming but rewarding task. Most teachers will set a more modest goal, easier to attain, and will, I hope, follow some (*but not by any means all*) of the suggestions. Practically all of the recommended reading is in works in English, although many excellent works exist, untranslated, in French, German, and Italian.

1. No Topic in the History of Science Can Be Understood in Isolation

I assume, at the outset, that we are not concerned here solely with the introduction of anecdotes,[4] but rather with a sincere desire on the part of the teacher to tell his students, from time to time, something about the development of physics—the way in which some of the major ideas were first formulated, how they were verified, and the mode of evolution of our present conceptual schemes.[5] This requires, on the part of the teacher, a sincere desire to know something more about the history of science than is available, say, in the historical paragraphs in the average physics textbook. For teachers there are available in English three short histories of physics,[6] as well as a number in German and French. A little reading in such works will help the physics teacher to learn something of the continuity in the growth of physics and the relation between different episodes.[7]

Most physics textbooks are particularly weak—on the historical side—when it comes to antiquity and the middle ages. The recent publication of the superb *Source Book in Greek Science* will enable teachers to remedy this weakness.[8] The complementary *Source Book in Medieval Science* now in preparation will help to complete the picture. The great difficulty in presenting ancient and medieval scientific material is that the courses we teach tend to establish a stereotype of science and something called "scientific method." When we examine early writings, we tend too often to measure them against our stereotype

[4] Anecdotes, especially those that can be substantially verified, are extremely useful in teaching. They tend to make great men less "austere" and more "human."

[5] Most physics teachers are acquainted with the noble pioneering efforts of the late Lloyd W. Taylor; his ideas are conveniently summarized in "Science in general education at the college level," *Science* 91, 560–565 (1940) and are implemented in his textbook, *Physics the pioneer science* (Houghton-Mifflin Company, Boston, 1941). An earlier attempt to accomplish this same end was made by the Dane, Poul La Cour (1846–1908), in collaboration with Jakob Appel, *Die Physik auf Grund ihrer geschichtlichen Entwickelung für weitere Kreise in Wort und Bild dargestellt. . . .* Autorisierte Uebersetzung von G. Siebert (F. Vieweg und Sohn, Braunschweig, 1905); the original was published in Danish, *Historisk fysik . . .* (Det Nordiske forlag, Köbenhavn, 1896–1897). An approach to science teaching making use of the study of historical "case histories" is described by James B. Conant, *On understanding science* (Yale University Press, New Haven, 1947). Descriptions of a number of courses in the physical sciences for non-scientists, using the history of science to varying degrees and in different ways, may be found in Earl J. McGrath, ed., *Science in general education* (Wm. C. Brown Company, Dubuque, Iowa, 1948).

[6] H. Buckley, *A short history of physics* (Methuen, London, 1927); Florian Cajori, *A history of physics* (The Macmillan Company, New York, 1929); Henry Crew, *The rise of modern physics* (Williams & Wilkins, Baltimore, 1935).

[7] Professor George Sarton of Harvard University has prepared a bibliography of general books and monographs dealing with all aspects of the history of science which will be published in the late spring of 1950 by the Chronica Botanica Publishing Company of Waltham, Mass. A complete list of biographies (in English) of engineers and scientists has been compiled by Thomas James Higgins, Professor of Electrical Engineering at the University of Wisconsin (and is available without charge from the Graduate School, Illinois Institute of Technology, 3300 Federal St., Chicago 16, Ill.), published as *Research Publications, Illinois Institute of Technology*, Vol. 7, No. 1 (May, 1949). A convenient pamphlet (35 pp.) is Samuel W. Geiser and Bessie T. Geiser, *A brief short-title list of published works on the history of science* (Southern Methodist University Studies, No. 1, University Press in Dallas, 1947. Price 75¢).

[8] Morris R. Cohen and I. E. Drabkin, *A source book in Greek science* (McGraw-Hill Book Company, New York, 1948). At the end of this volume, there will be found a guide to further reading in ancient science. A fascinating supplementary booklet (110 pages) is J. L. Heiberg, *Mathematics and physical science in classical antiquity* (Oxford University Press, London, 1922). For an interesting discussion of the practicality of using such material, see J. J. G. McCue, "Ancient science in the modern curriculum," *Am. J. Physics* 16, 404 (1948). See also Otto Blüh, "Did the Greeks perform experiments?" *Am. J. Physics* 17, 384–388 (1949).

standard. This approach denies the evolution of the canons of scientific knowledge, which were clearly different in the 4th century B.C., the 10th century of the Christian era, the 16th century, and the 20th century. Approaching this problem with a true sense of history, we would not ask the extent to which the writings of Plato and Aristotle might satisfy the canons of 20th-century science, but rather try to find out from a study of such writings what the canons were, for discussions of nature, in the 4th century B.C. and in successive later eras. The latter approach entails an obligation to know something about those eras in general, so that we may understand why their canons of "nature-knowledge" took the form they did. Such information may be obtained from books on general history, intellectual history, or the history of philosophy. Some information of this kind is provided in books on the history of physics (e.g., Buckley, Cajori, Crew) and general histories of science.[9]

A few examples of currently held misconceptions may make clear why one cannot treat historial episodes in isolation from the history of science in general. The common textbook account would have us believe that Aristotle, who was interested only in deductions made from *a priori* assumptions unrelated to the world of experience, based his physics on the supposition that bodies fall to the earth at speeds proportional to their weights, and that everyone believed what Aristotle said until Galileo dropped two unequal weights from the Leaning Tower at Pisa to confute the Aristotelians. Considering the prominence given to Aristotle's *dicta* concerning falling bodies, both in physics books and all too many histories of physics, the otherwise unprepared student who reads Aristotle's works cannot help but be astonished to find that in Aristotle one finds slight reference to this topic. The conclusion to which we are led is that, because of the importance of the subject of accelerated motion in the 17th century, and the rather central place that dynamics has occupied in modern physics, we have stressed, in our discussion of Aristotle, a point which was not stressed to the same degree by him.

To be sure, in this case we have an example of Aristotle's setting forth conclusions which would not pass the test of experience, or experiment. In our teaching we should emphasize this point, because it shows our students the principle mentioned earlier, that the canons of "science" or "nature-knowledge" were different in the time of Aristotle from what they were later, that the very practise of science and its standards of knowledge have undergone dramatic and significant changes. This becomes all the more apparent when we realize that Aristotle's work in the biological sciences shows him "at his best" from "the modern scientific standpoint." "His first-hand observations are on living things, and his researches on them establish his claim to be regarded as a man of science in the modern sense." Not only did he study carefully the life and breeding habits of a large variety of animals (about 540 species), but he made a series of "embryological investigations of the developing chick, which has ever since been the classic object for such investigations."[10] We have, then, a sort of paradox in Aristotle in that in his biological writings he proceeds much like the modern scientist, whereas in his physical writings he does not. Now, I submit that the students in our colleges would be much more interested in this curious state of affairs—when the methods of exact observation were applied by the great master to some fields and not others—showing clearly the youth of science, than those same students would be in the obviously misleading

[9] H. D. Anthony, *Science and its background* (Macmillan and Company Ltd., London, 1948), Sir William Dampier, *A history of science and its relations with philosophy and religion* (University Press, Cambridge, ed. 4, 1949), Sedgwick, Tyler, and Bigelow, *A short history of science* (The Macmillan Company, New York, 1939), Charles Singer, *A short history of science to the nineteenth century* (Clarendon Press, Oxford, 1941), F. Sherwood Taylor, *A short history of science and scientific thought, with readings from the great scientists from the Babylonians to Einstein* (W. W. Norton and Company, New York, 1949).

[10] Quoted from Singer, *Short history* (see reference 9), pp. 39, 43. Singer also points to Aristotle's "anatomical descriptions of the four-chambered stomach of the ruminants, of the complex relationships of the ducts and vessels in the mammalian generative system and of the mammalian character of the porpoises and dolphins, all unsurpassed until the sixteenth century," and his "accounts of exceptional modes of development of fish. Among them is one of a species of dogfish of which the young is linked to the womb by a navel cord and placenta, much in the manner of a mammal. Nothing has contributed more to Aristotle's scientific reputation in modern times than the rediscovery of this phenomenon in modern times." (From p. 44.) See also Singer's *Greek biology and Greek medicine* (Clarendon Press, Oxford, 1922). For an account of Aristotle stressing his biological interests, see Werner Jaeger, *Aristotle* (Clarendon Press, Oxford, 1934).

statement that Aristotle wrote largely about nature without ever observing it.

By stressing the youth of science, and calling attention to its later conceptual evolution, we can also avoid the error of thinking that our predecessors were not quite as smart as we are. It is true that our average college senior in physics today knows more physics than Aristotle or Archimedes, and perhaps even Galileo and Newton, but he isn't more intelligent. A sense of history in science demands, then, that we attempt to find out why such intelligent thinkers in the past came to conclusions that we today consider to be such "obvious" errors, but which cannot have been so "obvious" after all.[11] One part of Aristotle's discussion is most instructive. In the *Physics*, Aristotle "shows" that in a void (or vacuum), all bodies must move with equal velocity. Since "this is impossible," he concludes, there can be no such thing as a void or empty space.[12] Often in the development of physics the wrong conclusion is drawn from the evidence of experience or theoretical reasoning—a fact which shows us that science always progresses by halts and jumps and that there really is no magic key called "scientific method" (something that the social scientists all too often think the physicists have in their armory but won't share with them)[13] which guarantees the taking of the correct next step at the right time.[14]

In contrast to Platonism, Aristotelianism has always suggested an appeal to experience, to the real sense-data of the external world. The Platonic-Pythagorean tradition is associated with numbers,[15] idealized problems, and abstraction generally, as contrasted with the down-to-earth, hard-boiled, "realistic" quality of the Aristotelian tradition. Thus, in the famous painting by Raphael of the "School of Athens,"[16] Plato and Aristotle are depicted in the center, dividing the universe between them. Plato looks upward into the world of abstraction, while Aristotle looks downward at the earth with its concrete reality. The greatly oversimplified (and erroneous) picture of Galileo as[17] the initiator of the "experimental method" will be discussed later in this article, but at this point I would merely like to show how wrong it is to think of Galileo as the man who confuted the Aristotelians by his insistence on experiments and his reliance on experience, which factors were supposedly alien, or even hostile, to the tenets of Aristotelianism.[18]

In his *Two New Sciences*,[19] first published in 1638, three discussants participate, of whom one, "Simplicio,"[20] is an Aristotelian. Galileo investigates two possibilities: that in the motion of

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[11] For the "reasonableness" of Aristotle's conceptions, see *Source book in Greek science* (reference 8), p. 201.

[12] See *Source book in Greek Science*, reference 8, p. 206.

[13] Cf. P. W. Bridgman, *Reflections of a physicist* (Philosophical Library, New York, 1950), pp. 310–311.

[14] One of the most interesting examples in this regard concerns the fogging of photographic plates which were kept too near Crookes tubes—a frequent occurrence at the end of the last century. Lord Rayleigh tells us that Crookes himself was later annoyed at having missed the discovery of x-rays and having assigned the cause of fogging to faulty manufacture. But another British scientist traced down the fogging to its source, the active Crookes tube; and from this observation drew the following inference: photographic plates should not be stored near a Crookes tube in operation. For further details, see I. B. Cohen, *Science servant of man* (Little, Brown and Company, Boston, 1948), Ch. 3; Lord Rayleigh, "Some reminiscences of scientific workers of the past generation, and their surroundings," *Proc. Physical Soc.* **48**, 217–246 (London, 1936); G. Sarton, "The discovery of x-rays," *Isis* **26**, 349–369 (1937).

[15] In recent years, some authors—incensed by the later writings of Eddington—have attacked Plato (and his master Pythagoras) as the founder of numerology and the enemy of science, e.g., E. T. Bell, *The magic of numbers* (Whittlesey House, New York, 1946). As A. Koyré has pointed out, "Galileo and Plato," *Journal of the History of Ideas* **4**, 400–428 (1943), such authors usually ignore the important fact, stressed by Leon Brunschvicg in his *Les étapes de la philosophie mathématique* (Alcan, Paris, 1912), p. 69, that there are two distinct traditions stemming from Plato: the numerologists to be sure, and also those who like Galileo tried to apply mathematics to the physical world of experience (in other words, the mathematical physicists). Thus, Galileo himself writes: "That the *Pythagorians* had the science of numbers in high esteem, and that *Plato* himself admired humane understanding, and thought that it pertook of Divinity, for that it understood the nature of numbers, I know very well, nor should I be far from being of the same opinion." *Dialogue of the two chief systems of the world*, translated by Thomas Salusbury (William Leybourn, London, 1661), p. 3.

[16] See E. C. Watson, *Am. J. Physics* **16**, 115–117 (1948).

[17] It is of some interest to note that the first recorded account of the experiment of dropping two unequal weights and noting that "the ratio of the times required for the motion does not depend on the ratio of the weights, but that the difference in time is a very small one," is due to Ioannes Grammaticus (*or* Philoponus) who lived at the end of the 5th and beginning of the 6th century of our era. Cf. *Source book in Greek science* (reference 8), p. 220.

[18] The question as to whether Galileo was a true empiricist will be discussed further in the following section.

[19] Galileo Galilei, *Dialogues concerning two new sciences*, translated by Henry Crew and Alfonso de Salvio (The Macmillan Company, New York, 1914, 1933).

[20] The name "Simplicio" is the Italian form of Simplicios (6th century A.D.), the famous Aristotelian commentator; Galileo, a lover of puns, was motivated in his choice by the fact that "simplicio" is also the Italian word for "simpleton."

falling bodies, the speed (i) may be proportional to the distance the body has fallen from rest, or (ii) may be proportional to the time that the body has been falling from rest. He shows that the former alternative leads to a contradiction and, therefore, accepts the latter. On that basis, it is then shown "by simple computation that a moving body starting from rest and acquiring velocity at a rate proportional to the time, will, during equal intervals of time, traverse distances which are related to each other as the odd numbers beginning with unity, 1, 3, 5; or considering the total space traversed, that covered in double time will be quadruple that covered during unit time; in triple time, the space is nine times as great as in unit time."[21] To this Simplicio, the Aristotelian, replies that the conclusion must be accepted if one accepts Galileo's definition of uniformly accelerated motion, "But as to whether this acceleration is that which one meets in nature in the case of falling bodies, I am still doubtful; and it seems to me, not only for my own sake but also for all those who think as I do, that this would be the proper moment to introduce one of those experiments—and there are many of them, I understand—which illustrate in several ways the conclusions reached."[22] Now the point to be noted here is that it is the Aristotelian, Simplicio, who is concerned to know whether Galileo's definition of uniformly accelerated motion corresponds to the motions actually found in nature and who suggests the need of an experiment, or a recourse to experience.[23]

The same situation is exhibited to an even more marked degree in Galileo's *Dialogue of the Two Chief Systems of the World* (1632), when the question is discussed as to whether a stone let fall from the mast of a moving ship would fall "precisely in the same place on the ship at which it falls when the ship is still."[24] "Salviati," speaking for Galileo, asks of Simplicio the Aristotelian, "Have you ever made the experiment[25] of the ship?" To which Simplicio replies: "I have not made it. But I do well believe that those authors who use it in arguments have diligently observed it. . . ." Salviati then replies: "That it can happen that those authors cite it without having performed it, you yourself are a good witness, since without having performed it, you cite it as certain and rely in good faith on their statement. Likewise, it is not only possible but it must be that they themselves did the same thing; I mean, rely on their predecessors without there ever being anyone who performed it. Because anyone who will perform it will find that the experiment shows the very contrary of what is written; that is, it will show that the stone falls always in the same place on the ship, whether the ship is standing still or moving at whatever speed.[26] Wherefore, since the same reason applies to the earth as to the ship, from the fact that the stone always falls perpendicular to the foot of the tower, nothing about the motion or the quiet of the earth can be inferred."[24] Thus, Galileo posits, apparently, the two extremes: the Aristotelian who relies on books, and who assumes that the "facts" described in them refer to actual experiments or observations made by someone in the past, and the Galilean experimental physicist who insists on making his own observations and experiments. But we may note that Salviati does not reply to Simplicio by actually describing an experiment that was actually performed; rather, he tells Simplicio what *would* happen if one *were* to perform the experiment. And when, shortly thereafter, Galileo has Simplicio ask if the ship experiment has really been made, Salviati proudly replies: "I without experiment [experience] am certain that the effect will follow as I tell you,

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[21] See reference 19, p. 177.

[22] See reference 19, p. 178.

[23] In translating Galileo, the very serious problem arises of how to translate "esperienza," which can mean either "experience" or "experiment"—or perhaps both simultaneously. It is not always clear from the sense of the text—and perhaps it was not always clear to Galileo himself—in which occurrences "esperienza" implies only a general reference to the world of sense data, to the world of ordinary experience, and even to "what everybody knows"; and in which occurrences "esperienza" implies a particular set of operations designed to answer (or illuminate) a specific question (or topic). On the whole vexing question of translation, see Alexandre Koyré, "Tradutorre-traditore à propos de Copernic et de Galilée," *Isis* **34**, 209–210 (1943), Henry Crew, "Littera occidit—spiritus vivificat," *Isis* **34**, 300–301 (1943). The same problem arises in translating from the French ("expérience") and the Latin ("experimentum" or "experientia").

[24] Translated by Professor C. S. Singleton of Harvard University. Cf. Salusbury's translation (reference 15), pp. 125 ff.

[25] Or, "Have you ever had the experience of the ship?" etc. Cf. reference 23.

[26] Needless to say, "the stone" will not "fall always in the same place on the ship, whether the ship is standing still or moving at whatever speed," as Galileo might easily have discovered by simple, careful experiments or observations.

because it is necessary that it should."[27] In other words, the more strictly empirical of the two is the Aristotelian.

But if we now admit that the "classic" picture of Galileo as the man who relied on experiment and the Aristotelians as those who denied the validity of experience is not quite accurate, the question immediately arises as to what the poor teacher of elementary physics is to do. Clearly, if every time he makes a reference to an historical question, he must engage in extensive reading, he will have no time for anything else and will end up with the history of science as his exclusive occupation. A little further thought will, I believe, show that this need not be the case. All that is required is a little time devoted to reading and the exercise of a certain amount of caution. The chief hurdle is to get rid of the historically naive attitude that what one reads in any book is apt to be true.[28] In preparing a lecture that has some historical content, it surely is not too much to ask that the teacher invest 15 or 20 minutes to checking the accuracy of that material. As the teacher begins to develop a sense of historical skepticism, he will then become more cautious and, instead of relating as fact some episode that he has not yet had time to check, he will say, "Tradition has it that . . . ," or "There is a story to the effect that. . . ." A little historical checking over a period of two or three years will produce much more accurate lectures, and will prove—I have no doubt—an agreeable task to the teacher. The whole point is that teachers should become aware of the need of more accuracy in the historical parts of their lectures.

This is the first step. The second is to gain a fuller picture of man's intellectual development and the general growth of science—not just the history of physics—so that, even if the individual facts themselves are correct, they may be presented in their proper setting. As we have just seen, to understand the nature of Aristotelianism requires more than a bare, correct statement of Aristotle's conclusions about falling bodies. In order to prevent the distortion that necessarily arises when facts are presented in isolation, some solid reading in history is required in addition to the sort of checking I have described in the previous paragraph. Any general book on the history of science[9] will serve as a starting point, but I should especially like to call the attention of physics teachers to Herbert Butterfield's *Origins of Modern Science*,[29] a most stimulating book that comprises a series of lectures delivered in Cambridge University (England) that begin with late medieval dynamics and carry the story of the development of our modern science into the 18th century. Butterfield is a professor of modern history and his book displays the historian's craft in presenting the growth of scientific ideas as a unified, coherent entity. The reading of such a book will be a rewarding and pleasant experience for anyone interested in science and it will provide teachers of physics with the background of the most crucial and important era in the making of science as we now know it.

I would hope that physics teachers would include among their reading at least one book on the history of science a year, and perhaps also two or three articles on special topics that prove of interest.[30] In this way, within a few years, such a teacher will have begun to acquire a grounding in the history of science that will be of great profit to him in giving him a better understanding of the scientific enterprise as a whole and, at the same time, providing a source of enjoyable reading. The questions of how to find the best works on the history of science for further reading, and how to evaluate them, will be discussed in the latter part of this article.

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[27] In Salusbury's translation, published in 1661, Galileo is made to be more of an empiricist than is warranted. Although the Italian original reads plainly, "Io senza esperienza son sicuro che l'effeto seguirá come vi dico, perché cosi è necessario che segua," Salusbury omits the words "*senza esperienza*" and renders the above: "I am assured that the effect will ensue as I tell you; for so it is necessary that it should." Reference 15, p. 126.

[28] I am often astonished to find that many physics teachers will unquestioningly accept as true the *historical* statements found in elementary physics textbooks, even though they are apt to question some of the *scientific* content of the same book.

[29] Published in London by Bell, 1949, and distributed in America by The Macmillan Company.

[30] Teachers who develop an interest in the early periods may wish to read the "Legacy" series published by the Oxford University Press, comprising *The legacy of Egypt*, . . . *of Greece*, . . . *of Rome*, . . . *of Israel*, . . . *of Islam*, and, . . . *of the Middle Ages*. Each book in the series is composed of a series of essays devoted to such subjects as: mathematics, physical sciences, biological sciences, medicine, law, social structure, art, architecture, religion and theology, philosophy, etc. Thus, they provide an admirable introduction to a whole civilization and are, perhaps, the best source for removing the factors of isolation.

2. The History of Science Is a Living, Growing Discipline in Which Our Knowledge Is Continually Increasing

In most fields of knowledge, this principle is axiomatic. As proof that such is not the case in ours, we have only to note that the work most widely used by physics teachers is Ernst Mach's *Mechanics*[31] which was first published almost 70 years ago! It is not my purpose here to deprecate Mach's endeavor in any way; in fact, all of my graduate students in the history of science are regularly required to read it. Furthermore, the philosophical point of view adopted by Mach is of as great value today as it was 70 years ago and has had an important influence on physics and the philosophy of science, e.g., on Einstein.[32] In the period since Mach first published his *Mechanics*, we have learned a great deal about the history of this subject. In particular, we now know that Galileo did not, as Mach supposed, create the modern science of mechanics single-handed, and that his teachers and predecessors were not hidebound Aristotelians. Among the discoveries of Galileo's immediate predecessors, we may now include:

(a) The discovery in the 14th century of the theorem that the displacement produced by a uniformly accelerated motion in a given time is equal to that produced by a uniform motion equal to the mean speed during the same time interval.[33]

(b) The discussion in the 14th century of the acceleration of falling bodies, and the implication that they have an acceleration that is uniform in time,[34] and also the discussion of whether[35] the velocity of a falling body is proportional to the time of fall or the distance of fall.[36]

Mach's statement that the science of dynamics was practically the product of a single genius, Galileo, who "had to create . . . for us" the "entirely new notion . . . of acceleration"[37] has been widely repeated.[38] It is clear, however, that the concept of acceleration arose in the 14th century and was, therefore, available to Galileo. Furthermore, the theorem stated as (a) above, was demonstrated by Oresme, in the 14th century, as follows. The motion under uniform velocity in a given time may be represented by a rectangle such as $ABGF$ in Fig. 1, while the uniformly accelerated motion corresponds to the right triangle ABC.[39] Boyer tells us, "Oresme did not explicitly state the fact—this is, of course, demonstrated in the integral calculus—that the areas $ABGF$ and ABC represent in each case the distance covered; but this seems to have been his interpretation, inasmuch as from the congruence of the triangles CFE and EBG he concluded the equality of the distances." Furthermore, Boyer notes, "This is perhaps the first time that the area under a curve was regarded as a physical quantity, but such interpretations were to be-

[31] Ernst Mach, *The science of mechanics: a critical and historical account of its development*, translated by Thomas J. McCormack (The Open Court Publishing Company, Chicago, London, 1893, 1902, 1919, etc). The original German edition, *Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt*, was first published in 1883.

[32] Cf. Einstein's "Autobiographical Notes" in Vol. 7 of "The Library of Living Philosophers," Paul Arthur Schilpp, ed., *Albert Einstein: philosopher-scientist* (The Library of Living Philosophers, Inc., Evanston, Ill., 1949), pp. 21, 27, 29, 49, 53, 67, 69.

[33] See Carl B. Boyer, *The concepts of the calculus: a critical and historical discussion of the derivative and the integral* (Columbia University Press, New York, 1939; reprinted by Hafner Publishing Company, 1949), pp. 82–85; Marshall Clagett, "Some general aspects of physics in the middle ages," *Isis* **39**, 29–44 (1948); M. Clagett, *Giovanni Marliani and late medieval physics* (Columbia University Press, New York, 1941); George Sarton, *Introduction to the history of science*, Vol. 3, "Science and learning in the fourteenth century" [Williams & Wilkins (for the Carnegie Institution of Washington), Baltimore, 1947]. This important theorem received "an arithmetical or formally logical proof by the Englishmen: Richard Swineshead [or Suiseth] before 1350; John of Dumbleton (fl. at Oxford between 1331–1349); and Walter Heytesbury (mentioned variously at Oxford from 1330 to 1371). Oresme at Paris demonstrated this theorem graphically with singular neatness. The Italians of the 15th century leaned more heavily on the English logicians, but they were not unaware of the coordinate system of Oresme. It is this theorem with its geometric proof that Galileo applies to the free fall of bodies to derive his version of the law of free fall." Quoted from Clagett, *Isis* **39**, 39 (1948).

[34] E.g., by Jean Buridan (an exponent of the "new dynamics" at the University of Paris from 1329–1358) and later in 1545 by Domingo Soto, a Spaniard. Cf. Clagett, *Isis* **39**, 38–39 (1948) and Pierre Duhem, *Etudes sur Léonard de Vinci*, Vol. 3, "Les précurseurs parisiens de Galilée" (Hermann & Cie, Paris, 1913), pp. 555–562, also Duhem's article "Physics, history of" in *Catholic Encyclopedia*.

[35] E.g., by Albert of Saxony (1316?–1390). Cf. Clagett, *Isis* **39**, 39 (1948).

[36] For a biography of all these 14th-century figures, an account of their work, a list of their major writings, and a bibliography of the secondary literature concerning their achievement, see Sarton, *Introduction* (Vol. 3, described in reference 33). The three published volumes of Sarton's *Introduction* (covering the period from Homer to the end of the 14th century) provide a convenient summary of every aspect of the history of science up to the beginning of the 15th century.

[37] *Mechanics* (reference 31), pp. 133, 145.

[38] E.g., by Lancelot Hogben, *Science for the citizen* (W. W. Norton & Company, 1938), p. 241; etc.

[39] After Boyer, *Concepts* (see reference 33), p. 83.

come before long commonplaces in the application of the calculus to scientific problems."[40] Those who are acquainted with Galileo's account of uniformly accelerated motion in his *Two New Sciences* will recognize the similarity of Galileo's discussion and Oresme's. Theorem I, Proposition I states: "The time in which any space is traversed by a body starting from rest and uniformly accelerated is equal to the time in which that same space would be traversed by the same body moving at a uniform speed whose value is the mean of the highest speed and the speed just before acceleration began,"[41] and is proved geometrically in terms of a figure practically identical to that which we have reproduced as Fig. 1.

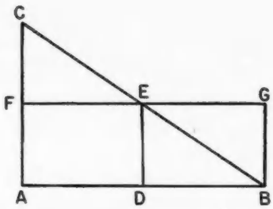

FIG. 1. Graphical representation of motion under uniform velocity and of uniformly accelerated motion.

Mach also represented Galileo as an experimentalist and states that after Galileo had worked out by reason and mathematics the relation between displacement and time for falling bodies,[42] ($s=\frac{1}{2}gt^2$) he obtained "experimental proof" of it by the experiment of timing a ball rolling down an inclined plane. Galileo is presented as one who "did not stop with the mere philosophical and logical discussion of his assumption, but tested it by comparison with experience."[43] Many writers today have recognized the error of thinking of Galileo as a strict empiricist, when he was nothing of the sort. To be sure, he made observations and performed rough checks of his results, but the "experimental method" of the "new science" was more accurately the creation of the next generation, including such heroic experimentalists as Robert Boyle.

In the previous section we saw that Galileo was hardly an extreme empiricist. But we must now ask how carefully did Galileo perform the experiment of timing the ball that rolled down the inclined plane? In fact, did he perform it at all? A careful reading and analysis of Galileo's writings[44] shows that he was much addicted to the "thought experiment,"[45] in which he imagined what the consequences would be if one did so and so. In the case of the experiment of the ball on the inclined plane, we do know that Galileo's contemporary, Father Mersenne, actually tried it in the hope of duplicating Galileo's confirmation of the supposed exact, direct proportion between the square of the time and the displacement in uniformly accelerated motion. In his book entitled *Harmonie universelle*, etc., Father Mersenne was forced to conclude, "Je doute que le sieur Galilée ayt fait les expériences des cheutes sur le plan." The reason: ". . . la proportion qu'il [Galilée] donne contredit souvent l'expérience."[46] Those who have attempted to perform this experiment for their classes know of the margin of error due to friction, rotational inertia, and the synchronization of the rolling ball and the timing device, and may well understand Father Mersenne's bewilderment when he compared the results of his own experiment with Galileo's statement that "the times of descent, for various inclinations of the plane, bore to one another precisely that ratio which . . . had [been] predicted. . . ." Also, ". . . there was no appreciable discrepancy in the results."[47] Galileo claimed

[40] See reference 33, p. 84.

[41] See reference 19, p. 173.

[42] Galileo never wrote the equation $s=\frac{1}{2}gt^2$ but dealt with proportions, e.g., Theorem II, Prop. II, "The spaces described by a body falling from rest with a uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing these distances." Quoted from *Two New Sciences* (reference 19), p. 174.

[43] *Mechanics* (see reference 31), p. 135.

[44] Much of the revision of our ideas concerning Galileo is due to the penetrating research of Alexandre Koyré, especially his *Etudes Galiléennes* (Hermann & Cie, Paris, 1939; "Actualités scientifiques et industrielles," Nos. 852, 853, 854), I: "A l'aube de la science classique"; II: "La loi de la chute des corps, Descartes et Galilée"; III: "Galilée et la loi d'inertie." Koyré has published two summary articles in English, "Galileo and Plato," *Journal of the History of Ideas* **4**, 400–428 (1943), "Galileo and the scientific revolution of the seventeenth century," *Philosophical Review*, 333–348 (1943). Another scholar whose work on Galileo is of foremost important is Leonardo Olschki, whose *Galilei u. seine Zeit* [Max Niemeyer Verlag, Halle (Saale), 1927] forms Vol. 3 of his monumental *Geschichte der neusprachlichen wissenschaftlichen Literatur*. Olschki too has published two summary articles in English, "Galileo's philosophy of science," *Philosophical Review*, 349–365 (1943), "The scientific personality of Galileo," *Bulletin of the History of Medicine* **12**, 248–273 (1942).

[45] The "thought experiment" of Galileo is particularly well described by Butterfield (see reference 29), pp. 71ff.

[46] Quoted in Koyré, *La loi de la chute des corps* (see reference 44), p. 147 [=II−73].

[47] *Two new sciences* (see reference 19), p. 179.

that the measurement of time was so accurate "that the deviation between two observations never exceeded one-tenth of a pulse-beat."

Our discussion above poses a very serious problem for the teacher whose sources for the history of science are generally limited and may include such works as Cajori's *History of Physics* and Crew's *Rise of Modern Physics;* textbooks with historical information such as Taylor's *Physics the Pioneer Science* and Preston's *Theory of Light;* and the *Encyclopaedia Britannica.* Granting that such a teacher wishes to make his account as accurate as possible, how, in the first place, can he possibly discover which recent works he should read? We shall return to this question presently. But the point to be made here is simply that old books and articles in the history of science are often likely to be, on certain issues, as hopelessly out of date and inadequate as older books on physics.[48]

3. While Books Provide Convenient Summaries, Monographs and Articles Must Be Used as a Supplement

In the history of science, as in science itself, the most reliable information is not to be obtained in books, but rather in the articles currently appearing in journals. Such articles are published not only in learned periodicals devoted to the history of science, such as *Isis,*[49] *Annals of Science,*[50] and the *Archives,*[51] in physics journals such as *The American Journal of Physics, Terrestrial Magnetism and Atmospheric Electricity, Journal of the Optical Society of America,* etc.; but also in general scientific journals such as *Nature.* The reason for this wide variety of vehicles of publication is threefold: (1) The journals devoted to the history of science are quarterlies of limited capacity. (2) Since only a small number of authors are professional historians of science, the greater number—the devoted amateurs to whom we owe most of our present knowledge of the history of science—prefer to publish their articles in journals primarily devoted to their own specialties or professional interests. (3) The professional historians of science, representing a young discipline, like to show their scientific colleagues the intrinsic interest and caliber of their writings and thus publish their articles in a large number of different journals so that their work will reach many scientists.

The average college library is apt to purchase a popular biography of a scientist, or a novel written about a scientist, but will not purchase a specialized monograph on late medieval dynamics. A work of the latter type is not only absent from the library, but probably will not even be reviewed in the journals that the average science teacher reads. It is clear, then, that the science teacher who wishes to learn more about the history of a given subject is in precisely the same position as the historian of science, since

[48] Naïveté about the history of science is not limited to scientists. Some years before the first World War, when George Sarton was in the throes of founding *Isis,* a journal devoted to the history of science, he hoped to obtain a board of patrons so that the new journal might be launched with more éclat than would be possible for an unknown Belgian scholar who had not yet won his spurs. A number of distinguished scholars and scientists accepted, but the distinguished French historian of religions, Salomon Reinach, refused. What would be the use of such a journal, asked Reinach, since the history of each of the sciences has been written by Ferdinand Hofer?—referring to a popular French writer of the late 19th century, whose works include volumes on the history of physics, chemistry, biology, astronomy, mathematics. When I decided to do graduate work in the history of physics, a relative of mine who was a mathematician asked me in the same terms what work there might be to be done since she assumed that there was already written a history of physics, just as there was a history of mathematics which he had read and enjoyed. The problem of research in the history of science is not only to make use of newly discovered manuscript material (especially true in the case of medieval science), but to reinterpret the work of older historians who were limited in outlook, to reassess various key figures in the science of the past whose contributions have been only imperfectly understood, to show the importance of neglected figures, and, above all, to tie in separate episodes in the history of science with the major streams of human history (philosophy, theology, literature, political theory, etc.) and to present the history of science as a continued, unified development.

[49] *Isis* is published in America by The History of Science Society. Membership in the History of Science Society entitles one to receive *Isis.* The founder and editor is Professor George Sarton of Harvard University, the managing editor is I. Bernard Cohen. Information about *Isis* or the History of Science Society may be obtained by writing to the Society at Widener Library 189, Harvard University, Cambridge 38, Mass.

[50] *Annals of Science,* formerly a quarterly, is now published irregularly by Taylor & Francis in London. Devoted to the history of science since the Renaissance, its editors include Professor Harcourt Brown of Brown University, Professor Douglas McKie of the University of London, and H. W. Robinson, Esq., formerly Librarian of the Royal Society of London.

[51] This journal was founded in Italy by the late Aldo Mieli as *Archivio di storia della scienza;* its name was subsequently changed to *Archeion* and it was published in France, later in Argentina; it is now called *Archives internationales d'histoire des sciences* and is published once again in Paris with a subvention from UNESCO. It is the official journal of the International Academy of the History of Science.

both have the problem of finding out what has been written on a given historical topic.

Fortunately, there is a relatively simple way to solve this problem, for which our thanks are due to George Sarton, the most venerable figure in the history of science, who has provided a key to the mass of published literature relating to the history of science in *Isis*, the journal which he founded in 1913, and which is today the official quarterly journal of the History of Science Society. With the collaboration of scholars all over the world, Sarton assembles a semiannual Critical Bibliography which includes both articles and books, listed by author in a number of alternative categories; *Isis* just published the 74th such Critical Bibliography. A teacher who is interested in gathering a list of the writings dealing with Galileo and his precursors in dynamics has merely to turn to the sections of the Critical Bibliographies covering the 14th, 15th, 16th, and 17th centuries, and the sections devoted to the history of physics and of mechanics.[52] As a matter of fact, it is not necessary to go through all the published Critical Bibliographies, because, if one locates four or five recent articles, the chances are that their authors will have canvassed the field and will provide references to other works that one may want to consult. Since all articles, notes, and book reviews published in *Isis* are also listed in the Critical Bibliographies, it is not necessary to scan the volumes of *Isis* or their indices in addition to examining the Critical Bibliographies.

Since *Isis* may be found in most university libraries, we see that it is possible to discover the monographs or articles that should be read. But what if it happens that the work in question, to take a real but extreme case, proves to be a pamphlet on 18th-century theories of combustion published a number of years ago in India! The situation is not then as hopeless as it might seem. Almost all of the articles and monographs listed in the Critical Bibliographies of *Isis* may be found in the Historical Apparatus assembled by George Sarton in Widener Library 185–189. This Apparatus is the most complete collection of books, journals, reprints of articles, and monographs devoted to the history of science ever to be assembled anywhere.[53] A loan of any of the works listed in the Critical Bibliographies can be readily arranged, or a microfilm or photostat copy provided. The Apparatus also includes a complete author index, so that if one encounters a publication by a given individual that is of interest, we can supply a complete list of that author's publications.[54]

Finally, if there seems to be no publication dealing with this or that topic, or answering a given question, a letter may be written to the Editor of *Isis*. If the editor, or one of the members of the editorial board, cannot answer the question, we will publish the letter in our "Queries & Answers" department. It is a matter of constant surprise how many such questions are answered in this way.

4. A Sense of Critical Discrimination Should Be Exercised in Choice of Reading

Part of the development of a sense of history in science is the ability to tell a good book from a bad one. A number of simple criteria are available. For instance, does the author give his sources? While this is not an infallible test, it often works; the authors of first-rate works usually give the reader the sources of information. Whether an article or a book, these sources—if given—help one to form an opinion in many cases. A book on the work of Helmholtz that makes no reference to German publications, but only to books and articles in English, is almost certain to be inadequate. An article on the development of the heat concept that has references only to secondary

[52] In the 74th Critical Bibliography, *Isis* **40**, 356–403 (1949), these sections occupy pp. 360–366, 387, and include about 60 items. Running through 74 Critical Bibliographies for such items would not take more than one or two hours at most.

[53] The assembling of this Apparatus is owing to a number of different factors. It includes complimentary and review copies of books sent to *Isis;* gift copies of books from the authors to George Sarton; reprints, monographs, etc., sent to *Isis* for inclusion in the Critical Bibliographies; journals received in exchange for *Isis;* books and journals purchased; and George Sarton's personal library. While Sarton was an Associate of the Carnegie Institution of Washington (until 1949), that organization maintained this Apparatus. More recently, the Apparatus has been given to the Harvard Library which has undertaken its maintenance and support.

[54] Another important tool is in the process of being completed at Cornell University under the direction of Henry Guerlac, Professor of the History of Science. This is a master Critical Bibliography, amalgamating all of the published entries in the Critical Bibliographies into a master series of cards, with certain older works of importance that were printed prior to the publication of the first volume of *Isis*.

works, implying that the writer has never read anything by the scientists he discusses, can hardly be very good. A pamphlet on Galileo that has references to no work published later than 30 years ago is hopelessly out of date. Historico-scientific novels about Kepler, Galileo, Tycho, or Galois may be interesting to read, as are science-fiction stories; but they should not be used as sources of history any more than we would use science-fiction stories as sources of information on jet-propulsion or atomic energy.

Unfortunately, as we have mentioned above, the level of accuracy in the writings on the history of science leaves much to be desired. In our field, all too many "outstanding" books by "eminent" authorities repeat old errors or create new ones. In many, but not all cases, the teacher can compare two or more different accounts of the same subject. I fear, however, that the average teacher of physics may, at this point, decide to "throw in the towel" and abandon the history of science altogether. This need not be the case, however. One way to evaluate a book is to look up a review of it (in *Isis*, or some other journal). Another is to test it on one or more issues with which one is familiar. Again, one can determine the author's preparation by examining the footnotes or bibliography, reading the preface, and gauging by the style how serious the author's intention was. The point is not so much to make detailed examination of every page of a book as it is to select those books that appear to be sounder than others. No one need do more than ruffle the pages of E. T. Whittaker's *History of the Theories of Aether and Electricity from the Age of Descartes to the Nineteenth Century* (Dublin University Press, 1910) in order to learn that it is a sound, serious, scholarly work of utmost reliability.[55] By contrast, if one finds that a book on the history of electricity has a chapter on Oersted written in the style of a 20th-century tabloid newspaper report, one may conclude that this chapter (at any rate) does not provide the most authoritative account of Oersted's work and its reception, although that chapter may provide interesting reading.

The whole problem resolves itself down to one simple fact: the physicist or teacher who wishes to use the history of science should be always aware that there are bad sources of information as well as good ones. I fear that all too many physics teachers just do not exercise any discrimination in choosing their sources of information, which may be the encyclopedias, textbooks of physics, popular biographies, serious biographies, and excellent historical works. A moment's thought to the separation of books and articles into their respective categories of reliability would help immeasurably.[56]

5. An Attempt Should Be Made to Check Occasionally Both the Plausible and the Implausible

Since many physics teachers like to tell anecdotes, it may be of interest to show how little trust can be placed in intuitive judgments of the plausible and the implausible stories about scientists. Of the two most famous anecdotes in the history of science, that of Newton and the apple and that of Galileo and the leaning tower of Pisa, —most people have agreed that the former is absurd and the latter very likely true.

Galileo surely dropped a pair of weights from a tower at some time in his life to find out whether, indeed, they fell at speeds predicted by the older physics; in his early days at Pisa, what better place would there be for such an experiment than the Leaning Tower! Yet, merely to have recognized that Aristotle was wrong in his statement that the speed of a freely falling body is proportional to the quotient of the weight of the body and the resistance of the medium can hardly be rated a great achievement, since, as we have seen, this goes back at least to John the Grammarian, 11 centuries before Galileo, whose writings were the starting point for the anti-

[55] Even so, a book 40 years old needs to be supplemented by references to more recent literature.

[56] Two sets of books of exceptional merit may be recommended to the teacher of physics. One is titled *Classics of scientific method*, edited by E. R. Thomas and published by G. Bell and Sons, Ltd., London. It includes Charles Singer, *Discovery of the circulation of the blood;* Clara M. Taylor, *The discovery of the nature of the air;* Alex Wood, *Joule and the study of energy;* J. R. Partington, *The composition of water;* Michael Roberts and E. R. Thomas, *Newton and the origin of colours.*

The second set, titled *Harvard case histories in experimental science*, is edited by J. B. Conant, and is published by Harvard University Press, Cambridge. Three volumes have been published, all in 1950: J. B. Conant, *Robert Boyle's experiments in pneumatics*, J. B. Conant, *The overthrow of the phlogiston theory:* Duane Roller, *The early development of the concepts of temperature and heat.* A fourth volume, L. K. Nash, *Atomic-molecular theory from Dalton to Cannizzaro*, is in press.

Aristotelian views on motion in the 14th century which we have discussed earlier.[57] Indeed, during his stay at Pisa, Galileo wrote his *De motu* (1590) in which his anti-Aristotelian ideas are in many ways like those of his 14th-century precedessors. In this work, Galileo says, "We will operate more with causes than with examples (for what we seek are the causes of phenomena which experience[58] does not reveal)," and also, "It is of no consequence, if occasionally experience contradicts the well-grounded theory."[59] Among the results of Galileo's experiments from "a tower,"[60] was the observation that "wood at the beginning of its motion is carried [*or*, put into motion] more quickly than lead; after a while, the motion of the lead is accelerated to such an extent that it leaves the wood behind it; and, if they are dropped from a high tower, will go ahead of it [the wood] by a long distance; and concerning this I have often made a test."[61]

We may conclude that in his Pisan days, Galileo did not really believe in the absolute reliability of experiment (experience), but claimed to have made some experiments with falling bodies from an unspecified tower (which may well have been the Leaning Tower); the experiments described may have disagreed with the laws of motion of Aristotle, but they also seem to have disagreed (at times) with what one would observe on repeating these experiments today.

So much for fact, now for fancy! As usually related, Galileo—with much previous advertisement—ascended the Leaning Tower of Pisa, dropped two balls of different material or of unequal weight, which, to the astonishment of an assembled multitude, hit the ground at the very same instant. Thus was born our modern experimental science. This story receives various embellishments in different books,[62] one of the most fantastic forms being that Galileo took a 1-lb weight and a 10-lb weight, enclosed them in separate, but identical, wooden boxes, etc. Those who wish to review the evidence for our doubting the whole story may consult Wohlwill's *Galilei-Studien* or Lane Cooper's *Aristotle, Galileo, and the tower of Pisa*.[59] But part of the evidence is that, of all the assembled multitude, no one left a record of this public experiment; it is not mentioned by Galileo's colleague at Pisa, Jacopo Mazzoni, an anti-Aristotelian, who published a work on Aristotle and Plato five years after Galileo departed from Pisa; nor is it mentioned by Giorgio Coresio, a professor at Pisa who published a work attacking the anti-Aristotelian views on motion of Mazzoni and Galileo; nor is it mentioned in the reply to Coresio written by Galileo's pupil, Castelli.

As an aside, may I say that this particular anecdote is not only without solid foundation, but it distorts the whole procedure of Galileo in demonstrating the laws of uniformly accelerated motion. Furthermore, it violates the principle we have stated earlier of presenting the gradual evolution of the concept of science. The importance of Galileo's work in dynamics was conceptual not experimental. He was not an empiricist in his Pisan days, and not completely an empiricist (in the sense that Boyle was) even in his later years. His achievement was to weld together the work of the anti-Aristotelians of the past two centuries or so, and to present a consistent, reasonable conceptual scheme that was descriptive rather than teleological, and that was not particularly concerned with final causes. His genius was such that his results were largely testable and were verified, and provided the basis for the final formulation in Newton's *Principia*.[63] The point is that the usual terms of the anecdote

[57] In this regard, see also R. B. Lindsay, "Galileo Galilei, 1564–1642, and the motion of falling bodies," *Am. J. Physics* **10**, 285–292 (1942).

[58] Or experiment (see reference 23).

[59] "De motu," *Le opere di Galileo Galilei*, Edizione Nazionale (Firenze, 1890), **1**, 263, 406. Cf. Emil Wohlwill, "Galilei-Studien," *Mitt. z. Gesch. d. Med. u. d. Naturwiss.* **4**, 229–248 (1905), **5**, 230–249, 439–464 (1906); Florian Cajori, "Falling bodies in ancient and modern times," *School Science and Math.* **21**, 638–648 (1921); Lane Cooper, *Aristotle, Galileo, and the Tower of Pisa* (Cornell University Press, Ithaca, 1935); Lane Cooper, "Galileo and scientific history," *Sci. Monthly* **43**, 163–167 (1936).

[60] Described "ex turri," "ex alta turri," "ab alta turri," in *De motu* (see preceding note), *Ed. nat.* **1**, pp. 263, 273, 334, 406, 407.

[61] Cajori's translation, *School Sci. and Math.* **21**, 644 (1921) from *Ed. nat.* **1**, p. 333.

[62] They have been collected and discussed in a most entertaining way by Lane Cooper in his *Aristotle, Galileo, and the Tower of Pisa* (see reference 59).

[63] The anecdote concerning Galileo's public demonstration from the Leaning Tower of Pisa is presented out of context in a singularly inept way by Anthony Standen in "Science can be silly," *Life*, **28**, No. 13, 105, 114 (March 27, 1950): "It is not Galileo's fault. When he dropped his two weights from the tower in Italy, he didn't know what he was starting. He started the modern science of physics. But according to the official account, he also started the modern scientific method."

violate a sense of history, since we are customarily told that the Aristotelians saw the experiment performed but would not believe the evidence of experience because they were Aristotelians.

By contrast to Galileo's public experiment from the Leaning Tower, the anecdote about Newton getting the idea of universal gravitation by watching an apple fall has always seemed less plausible. A British scientist has recently observed that "the story that a falling apple set him thinking about gravitation" is a part of the "ridiculous propaganda about his life."[64] For centuries, the only source of this story had been Voltaire's account, based on what he had heard from Mme. Conduitt, the niece of Newton. A number of historians of science, including Newtonian specialists, came to the conclusion that Voltaire was not a reliable source for the history of science and that, following the opinion of the great Gauss, we would have to be satisfied with Eve's apple in the Garden of Eden and that which Paris gave to Venus.[65]

But a few years ago, there was published from a manuscript in the Royal Society Library a biography of Newton written by the Rev. William Stukeley, F.R.S., in 1752. The author was himself an interesting figure[66] and he lived in Granthorpe, Newton's birthplace. On April 15, 1726, Stukeley spent the afternoon with Newton. He writes, "After dinner, the weather being warm, we went into the garden and drank thea, under the shade of some appletrees, only he and myself. Amidst other discourse, he told me, he was just in the same situation, as when formerly, the notion of gravitation came into his mind. It was occasion'd by the fall of an apple, as he sat in a contemplative mood."[67] Stukeley concludes, "This was the birth of those amazing discoverys, whereby he built philosophy on a solid foundation, to the astonishment of all Europe."[68] This newly discovered evidence does not prove that the story is true, but it does prove that Newton told it.

The plausible and the implausible may often be separated by applying operational criteria. For example, suppose we read that Eratosthenes, the 3rd-century Alexandrian, determined the radius of the earth with an extremely small error. Considering the means at his disposal, this seems a much better result than would be warranted, save for very good luck. An examination of his method shows that he assumed two cities, Alexandria and Syene, to be on the same meridian whereas Syene is 3° to the west of Alexandria, which raises our first doubt. We next ask what unit was actually used by Eratosthenes? The answer is the "stade." But how do we convert the "stade" into miles or meters? In other words, how do we find out how long the "stade" of Eratosthenes was? The answer is that we have no direct evidence whatever as to the length of the "stade" of Eratosthenes and can, therefore, make no accurate estimate of the accuracy of his result.[69] If one wonders about the true length of the "stade," one might consult a classicist or an ancient historian on one's faculty; he would be flattered to provide the answer.

A similar situation obtains in the case of Roemer's value for the velocity of light, in which we are given figures that range from 120,000 to 200,000 mi/sec or from 193,120 to 327,000 km/sec, with the most common value being 192,000 mi/sec, and 190,000 mi/sec running a close second.[70] Recalling that Roemer's method was based on observations of eclipses of Jupiter's satellites, we ask immediately how we convert his value into miles and kilometers. The discrepancy between the values assigned to Roemer by different writers arouses our initial doubt; the method itself, the second. Actually, Roemer gave no such value for the speed of light at all! He was satisfied to show that light travels with a finite speed, an assumption necessary to explain the otherwise unexplainable irregularity in the eclipses of Jupiter's satellites, and concluded that light requires "about 22 minutes" to traverse the orbit

[64] J. B. S. Haldane, *Science advances* (The Macmillan Company, New York, 1947), p. 11.

[65] Cf. B. Bessmertny, "Voltaire historien des sciences," *Archeion* **17**, 171–175 (1935); Léon Bloch, *La philosophie de Newton* (F. Alcan, Paris, 1908), p. 279. Gino Loria, *Scritti, conferenze, discorsi sulla storia delle matematiche* (Casa Editrice Dott. Antonio Milani, Padova, 1937), pp. 33 ff.

[66] Cf. I. B. Cohen, *Benjamin Franklin's experiments* (Harvard University Press, Cambridge, 1941), pp. 81, 83, 90–91.

[67] *Memoirs of Sir Isaac Newton's life, by William Stukeley, M.D., F.R.S., 1752*, ed. by A. Hastings White (Taylor and Francis, London, 1936), pp. 19–20.

[68] Cf. Jean Pelseneer, "La pomme de Newton," *Ciel et Terre*, 1–4 (1937); I. B. Cohen, "Authenticity of scientific anecdotes," *Nature* **157**, 196–197 (1946).

[69] Cf. Aubrey Diller, "The ancient measurements of the earth," *Isis* **40**, 6–9 (1949).

[70] These different values, as presented in modern works on physics and the history of science, are presented by Carl B. Boyer, "Early estimates of the velocity of light," *Isis* **33**, 26 (1941).

of the earth (2 astronomical units).[71] To convert Roemer's given value into a speed in miles per second most authors use a determination of the size of the earth's orbit (or the astronomical unit) that was unavailable in Roemer's day.

6. Some Reading in Original Sources, in Translation if Necessary, Is Required to Develop a Sense of History in Science

Clearly, the average teacher cannot be expected to read the original accounts of all the important scientific discoveries to which he will refer during his lectures. As a matter of fact, a professional historian of science could not do so for a course in the history of science. But the need for an occasional return to the primary sources is made especially evident by the relatively poor state of our secondary accounts. When two works on the history of science have contradictory accounts of the same piece of research, a good way to resolve the issue is to go back to the original. Obviously, the teacher of physics will not have time to do so often, but only on occasion.

I would urge, however, that teachers who like to use the history of science in their teaching do some reading of the great works they discuss. A selection of Faraday's "Experimental Researches" is available for about a dollar in the "Everyman" collection—a slim volume that fits easily into a coat pocket and that will provide excellent reading on the train. A few pages of Faraday, or Newton, or Galileo, or Franklin, or—for that matter—Aristotle, will give the teacher a better idea of that individual's actual work than a dozen books on the history of physics.

Here again, a certain amount of caution must be exercised. Most of the great scientific works have not been written in English and will, therefore, require being read in translation. In many cases, the translation may not have been made from the original. In the *Source Book in Physics*,[72] the most readily available collection, for example, some of the extracts are translated from a German translation of a Latin text.[73] We readily see that a sense of critical discrimination must be exercised in reading translations, therefore, much like that in evaluating biographies and histories which has been discussed earlier.

Another pitfall is the unedited text. Whether originally written in English, or whether translated into English, early works of science, and sometimes even those of a hundred years ago, cannot be fully understood by the uninitiated without some help with obsolete or obscure terms, and so on. It should be emphasized that the reading of original documents is a difficult task requiring a triple effort at the very minimum: to know (i) the correct or present view of the phenomenon under discussion, (ii) the author's approach to the problem insofar as it is discernible, and (iii) the contemporary state of knowledge when the account was written.

For example, reading Franklin's book[66] on electricity without any background in 18th-century science, may lead one to a number of erroneous conclusions. The first of these is that, noticing that the book is made up of letters, one may conclude that Franklin did not write any communications for publication in scientific journals. But an examination of the leading scientific journal of the day, the *Philosophical Transactions* of the Royal Society of London, where many of Franklin's letters were in fact published, would show that a large number of the contributions were in the form of letters, what Malpighi called "epistolary dissertations." Or, one might encounter the fact that the electric fluid postulated by Franklin was composed of mutually repellent particles. Shall we conclude that Franklin is the "discoverer" of the electron? To answer this question we must recall that Franklin's postulated "electric fluid" was but one of a number of "subtle, elastic" fluids common to late 17th- and 18th-century thought and including heat, light, etc. Such fluids were "elastic" because their component parts repelled one another; thus Newton attempted to explain the "elasticity" of the air by the repulsion between the "atoms" or air particles. The "atoms" of electrical fluid were not

[71] Boyer, *op. cit.*, has shown that even Roemer's figure of "22 minutes" is not reported correctly in many books on physics, the history of science, and optics. A facsimile of Roemer's original paper (in French) and the contemporaneous English translation may be found in I. B. Cohen, *Roemer and the first determination of the velocity of light* (The Burndy Library, New York, 1942; ed. 2, 1944); see correction in N. E. Dorsey, *J. Washington Acad. Sci.* **36**, 361–372 (1946); also *Isis* **39**, 56–58 (1948).

[72] Edited by the late William F. Magie (McGraw-Hill Book Company, New York, 1935).

[73] E.g., Galvani, pp. 420 ff.

"invented" in isolation; furthermore, to identify them with "electrons" is operationally untenable and is an example of reading history "backwards."

Among the less obvious traps for the unwary is the way in which the choice of simple words may imply wholly different scientific theories. The full title of Copernicus's great work *De revolutionibus orbium caelestium* (1543), is variously rendered: *Concerning the revolutions of the celestial bodies*, or . . . *of the heavenly orbits*, etc. Copernicus does not mean "planet" (*planeta*) by *orbis*, nor "body" (*corpus*), but rather "sphere," as has been fully demonstrated by the greatest living student of the work of Copernicus.[74] But does it make very much difference? Is it not pedantic to worry about such trivialities?

The ancient Greek astronomer Eudoxos (*c.* 408–355 B.C.) invented the theory of invisible (imaginary) spheres on which the observable planets were situated. The apparent motions of the planets were represented by the motion of these spheres, so that a planet had no motion of its own but moved only with the sphere which it was attached. Since a single sphere could not account for the nonuniform, observed motions, it became necessary to devise a whole set of such spheres for each planet. Until the discovery of the elliptical orbits by Kepler, astronomers had always resorted to a system of such spheres; some believing the spheres to be only a mathematical representation to "save the phenomena" and others granting the spheres a real crystalline existence. Now Copernicus "avoided taking sides in the controversy over the question whether the spheres were imaginary or real,"[75] and we may note that such a decision has no effect on the actual computations. But he did accept the doctrine of the spheres, and hence entitled his book *Concerning the revolutions of the heavenly spheres.* To render the title as *Concerning the revolutions of the heavenly bodies* (or *planets*) connotes a quite different theory of motions of individual bodies not dependent on spheres to which they are attached, and actually attributes to Copernicus a modern (post-Kepler) point of view that is both anachronous and highly misleading.

[74] Edward Rosen, translator and editor, *Three Copernican treatises* [The *Commentariolus* of Copernicus, *The Letter against Werner, The Narratio prima* of Rheticus] (Columbia University Press, New York, 1939), p. 19.

[75] Rosen, reference 74, p. 11.

Therefore, to read selections from Copernicus in which one key word may be wrongly rendered is apt to have a worse result than to read a good secondary work, e.g., A. Armitage's *Sun, stand thou still.*[76]

Conclusion

In the foregoing pages, I have attempted to show how a sense of history in science may be developed, and to indicate the simplest way of going about this task. The job is not, indeed, too arduous and will provide a source of stimulating and continually fascinating reading for the physics teacher. Sound caution and healthy skepticism with regard to common historical statements will prevent the repetition of many commonly occurring errors. As the teacher's horizon in the history of science expands, his historical references will take on a new significance which will make his lectures richer and more profound and of greater interest to his students.

As the physics teacher's interest in the history of science grows, I would hope that he might be tempted to try his own hand at historical writing, especially the modern or recent history of his own field of specialization. The future writing of a history of the science of our own time will be possible only if such historical articles are written now by those individuals who have the special experience and training to qualify them for the task. Even the recording of anecdotal material about one's colleagues and teachers will be of great importance for the understanding of the personality of the men of science of our own or the last generation.

The three papers which follow, which with the present one composed a symposium at the February, 1950 meeting of the AAPT, illustrate further aspects of the history of the science. Professor Boyer of Brooklyn College presents an exemplary study on the early history of the rainbow in which information of a kind not to be found in any secondary work is brought to light for the first time. This is followed by an interesting paper of Professor Brown of M. I. T., in which the supposedly "well-known" caloric theory

[76] (Henry Schuman, New York, 1947), one of a series of excellent readable works on the history of science by eminent scholars published under the general title, "The Life of Science Library."

is shown in its full mature dress and turns out to have been a much better theory than many of us would have supposed according to the usual account. Incidentally, we may note that while Professor Boyer's aim has been to uncover hitherto unknown material, Professor Brown's aim has been to show explicitly how a "case history" in the development of physics can be used to sharpen the wits of our students and to make them think about physics in a critical way. Finally, Professor Henshaw of Colgate University addresses himself to the topic of whether his students have found the history of science interesting. Not all teachers will agree with every point made by Professor Henshaw and I hope that the editor of the *American Journal of Physics* will be able to print many comments on this vital topic.

Each of these three papers illustrates the fact that is well known to historians, namely, that the pursuit of the history of science is difficult and arduous. The reward, however, is in proportion to that effort. While the muse Clio is a hard taskmistress, yet she grants to her followers a more mature and deeper understanding than would be otherwise possible.

A. G. Worthing Memorial Award

The Physics Department of the University of Pittsburgh has established the A. G. Worthing Memorial Award to be given annually to an outstanding senior student in physics who has a record of excellent scholarship and shows promise of achievement in the future in the field of physics. This carries a cash prize of $100.00, which comes out of a fund set up by contributions made by friends of the late Dr. Worthing. The first award was made this year to William M. MacDonald, a senior at the University.

This award was made at the annual Scholars Day ceremonies held by the University on April 21, 1950.

Kepler's Explanation of the Rainbow*

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IN a well-known work on optics one reads that "The first theory of the rainbow was given by Descartes in 1637."[1] Another recent author, likewise ascribing the first scientific explanation to Descartes, says that until the middle of the seventeenth century the theory of the rainbow remained in the domain of poesy.[2] Such views are, of course, as naive as they are widely held.

To begin an account of the rainbow with the year 1637 conceals the fact that serious explanations had been attempted over a period of two thousand years. It is not the intention here to survey in detail the developments within these two millenia; the aim is to call particular attention to a portion of the history of physics which appears not to be well known.

One of the very earliest naturalistic and mathematical theories of the rainbow was that given by Aristotle. Accounts of this explanation, based upon reflection from a cloud, can easily be found in books and periodicals devoted to the history of science.[3] It is intriguingly complicated—making use for the first time of the locus known as the circle of Apollonius—and has evoked numerous commentaries from his day to this. Among the commentators in the medieval age were Alhazen, an Arab, and Witelo, a Pole. Witelo (and possibly also Alhazen) knew of the need for refraction as well as reflection, but did not develop this idea. The modern approach to the rainbow through reflection and refraction of light within the individual drops of rain appeared, independently, in the works of two men who were close to each other in time but far apart geographically—the Teutonic Dominican theologian Dietrich (or Theodoric) of Freiburg and the Persian astronomer and physician Qutb al-dīn al-Shīrāzī.[4] The close similarity of the views of these men, both of whom died in 1311, or shortly thereafter, and their resemblance to the ideas of Descartes more than three hundred years later is a striking coincidence. Although there is no available evidence to show that Dietrich or Qutb al-dīn influenced either Descartes or his immediate predecessors, nevertheless the work of Theodoric was extant for some time in Germany. Regiomontanus thought of publishing it and the theory was taught at the University of Erfurt until the beginning of the sixteenth century. Thereafter it was lost and not rediscovered until Venturi ran across one of two surviving manuscript copies and published it in 1814.[5] European scholars of the early sixteenth century appear to have been familiar with the science of the fourteenth; but with the rise of humanism, interest in medieval thought declined, and this may account for the neglect of Dietrich's work. The ideas of Qutb al-dīn were developed by his student, Kamāl al-dīn (d. *c.* 1320), in a commentary on the optics of Alhazen; but the new theory seems to have had little influence, and a fresh start had to be made.

Early in the sixteenth century, just as Dietrich's work disappeared, there arose a wave of renewed interest in the theory of the rainbow, and this continued for at least two hundred years. It is probably safe to say that more volumes on the rainbow appeared between 1500 and 1700 than during all the years which preceded or succeeded; and most of these were pre-Cartesian, many appearing in Germany. Kepler lived during the very middle of this period—from 1571 to 1630. Could he have ignored these works devoted to one of the most striking of optical phenomena? After all, what Gilbert was to electricity, or Galileo to dynamics, or what Boyle was to pneumatics, that

* The second paper of a Symposium "Use of Historical Material in Elementary and Advanced Instruction," presented during the annual meeting of the American Association of Physics Teachers, Barnard College, New York, February 4, 1950.

[1] Robert W. Wood, *Physical optics* (The Macmillan Company, New York, new ed., 1928), p. 342.

[2] J. Cabannes, "L'explication scientifique de l'arc-en-ciel," *La Science Moderne*, **8**, 217–226 (1931).

[3] See, for example, Fr. Poske, "Die Erklärung des Regenbogens bei Aristoteles," *Zeitschrift für Mathematik und Physik, Historisch-literarische Abteilung*, **28**, 134–138 (1883); A. Sayili, "The Aristotelian explanation of the rainbow," *Isis*, **30**, 65–83 (1939); T. L. Heath, *Mathematics in Aristotle* (Oxford, 1949); Josephus Blancanus, *Aristotelis loca mathematica collecta et explicata* (Bononiae, 1615).

[4] See George Sarton, *Introduction to the history of science* (3 vols. in 5, Baltimore, 1927–1948), II, 761, III, 704–708.

[5] Giambatista Venturi, *Commentarj sopra la storia e le teoria dell' ottica*, vol. I (only one published), Bologna, 1814, pp. 149–180.

was Kepler to dioptrics. It therefore seemed incredible that Kepler should not have attempted some explanation; yet historical works on optics and the rainbow make virtually no mention of his views.[6]

A search of his works, however, reveals that Kepler did indeed make a determined effort to explain the rainbow—a study almost as earnest as his efforts, far better known, to discover the law of refraction. And one of the striking characteristics of his search is that the development of his own ideas as an individual follows closely the stages found in the wider history of the theory as developed by mankind at large. So close, in fact, is the resemblance that one is almost tempted to see here the operation of a general principle of mental development somewhat akin to the biogenetic law of recapitulation.

Kepler's interest in the rainbow seems to date from about the time of his *Mysterium Cosmographicum* (1596), when faith in the mathematical harmonies of the universe appeared to have been strikingly vindicated. Shortly after the publication of this work had launched Kepler upon a successful astronomical career, he sought to extend the harmonies of astronomy and music to include the phenomena of color. In marginal notes to the *Mysterium*, written apparently toward the very close of the century, the range of colors in the rainbow is compared to the infinity of tones in the musical octave. Yellow is taken as a sort of mean; and from this one passes, outward, through red into black as the solar influence diminishes and the admixture of the crass material in the cloud increases. From yellow inward one passes through green, blue, purple, and violet into black, and this transition is due to quite another cause, namely, refraction. Kepler adds that often he had considered whether or not the proportion of the angle of refraction determines the limits between the colors green, blue, etc. Direct vision, in which the angle of refraction is zero, results in yellow light; and when the angle of refraction is a right angle, all light ceases, so that this corresponds to blackness. But, added Kepler, how the greatest angle of refraction is to be subdivided in terms of color, it is difficult to say. Nevertheless, he believed that if the right angle were divided into parts corresponding to the simple unit fractions $\frac{1}{6}$, $\frac{1}{5}$, $\frac{1}{4}$, $\frac{1}{3}$, and $\frac{1}{2}$, the five colors would be yellow, green, blue, purple, and violet respectively. "And lo, is not the magnitude of the rainbow always about 45°, which is the measure of half of a right angle?" Kepler added cautiously, however, "But these may be notions."[7]

This, Kepler's early explanation of the rainbow, is a strange mixture of Aristotelian color theory and Pythagorean numerology. Had he thought of Occam's razor, he would scarcely have postulated two distinct causes of the rainbow. It will be noted further that the explanation is Aristotelian in the tacit assumption that it is the cloud as a whole which causes the phenomenon. Kepler seems to have been completely unaware of earlier theories (such as that of Johann Fleisher (1539–1593)[8] explaining the bow in terms of individual spherical drops. It is astonishing how poor scientific intercommunication of the time appears to have been. One wonders whether the radius of 45° which Kepler, throughout his life, accepted for the bow, was determined independently or was borrowed from others. Kepler cites no authority for it.

The early views of Kepler on the rainbow were immature in the extreme, and yet he hastened to publish them. In an astrological treatise of 1602[9] he reiterated the Aristotelian distinction between reflection from the surface of a mirror, in which form is preserved, and reflection from an uneven

[6] Alfred Kunze, *Zur Geschichte der Theorie des Regenbogens* (*Jahresbericht über das Karl-Friedrichs Gymnasium zu Eisenach* (Eisenach, 1870), does not mention Kepler. J. C. Sturm, *Thaumantias, sive iridis admiranda* (Norimbergae, 1699), apparently knew only of Kepler's work of 1604. Etienne Montucla, *Histoire des mathematiques* (new ed., 4 vols., Paris, 1799–1802), devotes but a paragraph to Kepler's letter to Harriot (see vol. I, p. 702). Henri Brocard, the eminent geometer, devoted a long account to Kepler's metereology ["Essai sur la météorologie de Kepler," *Bulletin de la Société de la statistique de l'Isère*, Grenoble, series 3, **8**, 360–400 (1879); **10**, 281–313 (1880); but only a few paragraphs treat of the rainbow, and these do not touch upon his geometrical explanations. The Kepler *Festschrift* edited by Karl Stöckl, *Bericht des Naturwissenschaftlichen Vereins zu Regensburg*, 1928–1930, Heft 19, contains excellent papers on Kepler's dioptrics but includes nothing on the rainbow. Two excellent little works, one by Friedrich Just, *Geschichte der Theorien des Regenbogens* (Marienburg, 1863), and the other by Reclam, *Über den Regenbogen* (Neustettin, 1877), mention letters of Harriot to Kepler but not those of Kepler to Harriot.

[7] Johann Kepler, *Opera omnia* (ed. Ch. Frisch, 8 vols., Francofurti a. M. and Erlangae, 1858–1870), I, p. 200.

[8] *De iride doctrina Aristotelis et Vitellionis certa methoda comprehensa* (1571). I cite this on the basis of the article "Regenbogen" in J. S. T. Gehler's, *Physikalisches Wörterbuch* (new ed., vol. 7, part 2, Leipzig, 1834), pp. 1318–1340.

[9] *De fundamentis astrologiae certioribus* (Pragae, 1602). See his *Opera*, **1**, p. 425.

surface, in which the light is imbued with color. Pursuing this thesis, he held, somewhat as had Seneca, that the colors of the rainbow fall into two classes: those arising from the darkening or privation of light; and those from refraction or tincturing. Kepler here seems to use the term refraction in the Aristotelian sense of reflection. Of the former class, the first color is the light of white heat itself, which cuts the circle of the rainbow as if in two. On the one side the intensity of illumination diminishes, producing first yellow, then red, a darkish color, and finally black; on the other side it is refracted and reflected, resulting in green, blue, purple, and, finally, dark violet.

Kepler's views of 1602 still were strongly tinged with Peripateticism; and two years later he published, in his classic commentary on the optics of Witelo,[10] a crude qualitative modification, based upon refraction, which had been suggested by medieval commentators on Aristotle. Here Kepler again reiterated the idea that the colors of the rainbow result from two distinct causes—the attentuation of light and the injection of aqueous material—and he asserted categorically that the diameter of the rainbow is always 90°. Then he said that the bow is due to the refraction of the rays of light by rain or aqueous material *between the spectator and the sun*. It is therefore not true, he argued, that the rainbow is caused by the reflection or refraction of the rays of the sun or of vision in that portion of the cloud in which the bow appears—repeating a proposition which had long been argued pro and con in scholastic circles. In this same treatise Kepler naively cited the colors in the rainbow as supporting his quasi-medieval belief that comets and novae are aqueous in origin and nature. In a description by Cornelius Gemma Frisius (1535–1577) of the variations of color in the nova of 1572, Kepler had read that the new star was at first red in color, then became brilliantly yellow, then green, and finally disappeared after having taken on a violet color. But this is precisely the order of the colors in the rainbow, which results from humidity in the air; and from this Kepler felt that one may reasonably conclude that the star was engendered by humidity!

Kepler apparently was soon convinced that his views of 1604 were untenable, as his correspondence in the following year indicates. David Fabricius (d. 1617) had written him asking why lunar haloes are always half the apparent radius of the rainbow, i.e., half of 45°. Kepler replied that he could not say more of the rainbow and halo than he had given in his *Optics* [the *Paralipomena* of 1604] except that he had erred in placing the source in the sublime region far above the cloud, and that the cause lay in the drops or vapors in which the bow appears.[11] In this same year Kepler made his first crude attempt to explain the rainbow geometrically in accordance with his revised views and to answer the question raised by Fabricius. Writing to Johann Georg Brengger, professor of medicine at Kaufbeuren,[12] Kepler said one sees clearly that for the formation of the rainbow it was not so much rain which was necessary as it was the disposition of the air to collect into drops. His explanation, nevertheless, is based, not upon individual drops, but upon the spherical shape of the cloud as a whole. Let the rays of the sun, regarded as parallel, be given by the lines HDC, IE, and $KFAB$, and let the points B, C, D, F lie on a circle with center at A (Fig. 1). If the eye of the observer is at A, then the angle of the rainbow, angle BAC, is 45°. Kepler apparently followed Aristotle in assuming that in reflections causing color, the law of the equality of angles need not be satisfied. Therefore, Kepler argues, inasmuch as the rainbow and the halo are both due to refraction, were the eye to be placed at C, the sun would be observed directly along the line HDC and the halo would be seen along the ray CFK. But from geometry the angle DCF is precisely half the angle BAC, so that the radius of the halo is $22\frac{1}{2}$°. Years later Pierre Gassendi (1592–1655) likewise was unable to resist the temptation of seeing in the relation between central and inscribed angles the fact that the apparent size of the primary rainbow is double that of the ordinary halo.[13]

Kepler realized that his explanation gives rise

[10] *Ad Vitellionem paralipomena quibus astronomiae pars optica traditur* (Francofurti, 1604). This is found also in *Opera*, **2**, pp. 119–397.

[11] *Opera*, **2**, p. 100.

[12] For the little that is known of his life see Kepler, *Opera*, **2**, p. 37.

[13] See Gassendi, *Opera omnia*, (6 vols., Florentiae, 1727), **2**, pp. 86–93.

to many questions. Why, for example, should one not see a rainbow of radius 45° about the sun [presumably between the lines HDC and KAC],[14] or a halo of radius $22\frac{1}{2}$° opposite the sun [allowing the ray EI to be extended to touch the circle a second time and be reflected to A]? Or does the surface of the air really have the spherical figure assumed in the explanation? The nebula in which the bow is seen is usually agitated by rapid wind and does not preserve any particular form, whereas the rainbow is always circular. Moreover, the water and drops descend swiftly in paths which are not always straight lines. Kepler is unable to explain why such irregularities do not manifest themselves in the shape of the rainbow, except to suggest that it must be the body of the aqueous air, rather than the surface, which serves as the agent.

Kepler, in his correspondence with Brengger, makes a number of other suggestions, some of which are wildly imaginative. Inasmuch as halos are seen generally through fine mists, whereas rainbows appear during a downpour, Kepler asks whether refraction in air in which drops are just beginning to be formed may not be half as great as that in air in which rain is already falling. Perhaps, on the other hand, the double radius of the rainbow is due to the combined effect of refraction and reflection, the halo resulting from refraction alone. He seems to be unable to decide whether it is water or a shower of raindrops or simply aqueous air which causes the rainbow; and he asks whether the bow is peculiar to each observer or is due to the coloring of the whole mass of the air in question. Of one thing he is certain, however, and that is that such phenomena are not optical illusions, but come to the eye by real rays; for he himself has admitted into his room light from parhelia and has seen the colors on the wall. Then Kepler brings his feet to earth and suggests to his correspondent that he examine the refraction of solar rays through a spherical globe of water. Is it not true that rays traversing near the center of the globe are not colored while those passing near the edge are? Color therefore seems to depend upon the magnitude of the angle of incidence. Kepler's letter closes, however, with a

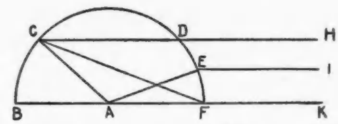

FIG. 1. Kepler's early explanation of the production of the rainbow by reflection from a spherical cloud.

frank admission of failure—what can one say about the origin of haloes and the rainbow? "I don't know," is his answer.

It was almost three years before Brengger replied to the letter of 1605, and in this interval Kepler developed his ultimate explanation of the rainbow.[15] On October 2/11, 1606 Kepler wrote to Thomas Harriot (1560–1621), an Oxford mathematician and scientist, proposing problems in optics and mechanics. (Harriot is one of the earliest scientists to have visited America, for in 1584 he accompanied Sir Walter Raleigh on his expedition to survey and map the Virginia territory.) Kepler had heard of Harriot's work in chemistry and optics (as well as his criticism of astrology!) and hence he requests the latter's views on the *Paralipomena*. If Harriot will but tell him the cause of color in refraction and send him the measures of refraction in his experiments, Kepler believes that the explanation of the rainbow will be much expedited. Then follows a lengthy description of Kepler's views on the rainbow, views contrasting sharply with those of the previous year. The demonstration of the rainbow, he holds, depends not on the cloud as a whole but on the smallest elements of it—tiny drops of rain which are exactly round. Kepler finally had reached the point at which his predecessors had arrived three centuries before. Just how he came to this view is not made clear, but it appears likely that he had followed his own advice to Brengger to study refraction in a spherical globe of water—a step which later led Descartes to success. Inasmuch as rays near the center of the sphere are not colored, while those passing near the edge are brightly tinted, Kepler made the assumption that the solar rays which produce the rainbow are those which strike the drop along a line of tangency. If, for example, S is the sun and O_2 the eye of the observer (Fig. 2), he assumed

[14] Kepler would have been greatly pleased to know that there is indeed about the sun a halo, not often seen, of radius about 46°.

[15] *Opera*, 2, pp. 67–71; This is found also in Kepler's *Epistolae* (ed. by M. G. Hansch, Lipsiae, 1718), letters nos. 152, 223.

that the tangent ray SA would be refracted along the line AB, then reflected from the concave surface of the drop along BC, and finally at C leave the drop refracted along the tangent line CO_2. Now Kepler assumed that the radius of the rainbow is 45°, and hence the arc AC must be 135°. Therefore $AB = 67\frac{1}{2}°$ and $RAB = 33\frac{3}{4}°$. Here one has a beautifully clear and simple explanation of the bow; but Kepler noted at least one fly in the ointment. The angle $33\frac{3}{4}°$ is too small, for tables of refraction indicated that the angle should be at least 37°. (For such a refraction the radius of the bow, under Kepler's explanation, would be about 32°.)[16] Kepler rather half-heartedly suggests that perhaps lukewarm rainwater, being less dense than our standing waters, may cause a lesser degree of refraction. He anticipates scepticism with respect to the tangency requirement for the solar rays and reiterates that only thus are colors formed.

The faith Kepler placed in his plausible explanation was confirmed by the fact that, with some modification, it served equally well for haloes. Not all of the ray AB is reflected at B, for a portion of it passes through the transparent surface, undergoing refraction along the tangent line. If the eye were at a point O_1 on this line (Fig. 2), it would see a halo of radius $67\frac{1}{2}°$, whereas experience shows that the actual radius is only $22\frac{1}{2}°$. To account for this one could assume that the angle SAB is $168\frac{3}{4}°$, but then how much less dense would be the substance causing the halo than that which produces the rainbow! Even Kepler hesitated here; and so he devised an alternative explanation for the halo which is based upon the same refractive index as is that for the rainbow. A portion of the ray BC, which causes the bow

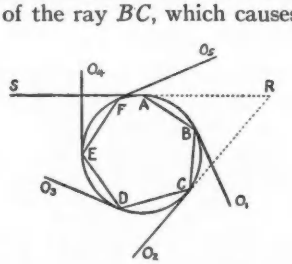

FIG. 2. Kepler's later explanation of the production of the rainbow by a combination of reflection and refraction from spherical droplets.

[16] Montucla, reference 6, incorrectly says that the *diameter* of the bow would be only 14°24′.

seen from O_2, is again reflected internally along CD and leaves the drop, after undergoing another refraction, along DO_3 (Fig. 2). This would cause a halo or rainbow of radius $22\frac{1}{2}°$ in opposition to the sun. The fact that this is never seen Kepler ascribes, rather inadequately, to the fact that it would be visible only when the sun is near the horizon—i.e., within $22\frac{1}{2}°$. Now a portion of the ray CD will undergo a third internal reflection, followed by a refraction at E along EO_4. This should cause a bow or halo of radius 90°, but this likewise is never seen. Finally, part of the ray DE is reflected a fourth time and leaves the drop at F, undergoing a refraction along FO_5. This ray does indeed make, with the solar rays, the required angle of $22\frac{1}{2}°$!

Kepler for several reasons hesitates to accept his beautiful pinwheel theory. In the first place, he thinks it incredible that the colored light, following so many reflections, would reach the eye in sufficient strength to cause an impression of color. Then, too, he found it difficult to reconcile the mobility of the falling drops with the constancy of the circular arch of the bow. Perhaps, he suggested, the bow is caused principally by dew. After all, there are, besides the common bow, also many kinds of extraordinary rainbows; and, after describing one of June 10/20 seen at Mogontia, Kepler closes with the challenge: "Thou, then, oh excellent priest of the mysteries of nature, tell the causes."

Harriot replied from London on December 2/11; but the reply must have been a disappointment to Kepler. The bulk of the letter is made up of a table of refractive indices of more than a dozen substances and an attempted explanation of the simultaneous reflection and refraction of rays by transparent media. Of the rainbow Harriot says only that when he writes on this he will give the proximate and immediate causes, for these are not correctly explained by the Peripatetics. He asks Kepler to be patient, adding: "I would say this of the rainbow just now, that the cause is to be demonstrated in a droplet through reflection on a concave surface and refraction on a convex. However, I have said nothing in consideration of the mysteries which are concealed."[17]

[17] Kepler, *Opera*, 2, pp. 71–72. See also *Epistolae*, pp. 376–378.

Kepler answered from Prague on August 2/11, 1607 that he was filled with eagerness to see Harriot's works on colors and the rainbow. They are in apparent agreement that the arc is caused by the individual drops and that the colors are produced by reflexion on the concave surface and refraction on the convex; and so Kepler hopes to receive a more adequate reply to his first letter.[18] It was almost a year before Harriot wrote again, on July 13/22, 1608. He pleaded lack of time for writing and philosophizing. He reported on some arguments for the existence of a vacuum which had been directed by William Gilbert (1544–1603) against the Peripatetics; but he said nothing further about the rainbow.[19] Kepler in turn delayed over a year before sending a reply on September 1, 1609, in which he argued against the possibility of a void.[20] The correspondence between them appears to have been broken off here, and we have no further information on Harriot's theory of the rainbow. Kepler meanwhile had resumed correspondence with Fabricius on November 10, 1608, giving some of his latest thoughts on the rainbow. Contrary to his previous letter of some four years before, he cites it as certain that the cause is to be found in the individual drops, and that the halo likewise is caused by the very smallest dew drops. He adds that he can give a beautiful cause for the fact that only the arc, and not its interior, is colored. Here he clearly has in mind the explanation which he had sent to Harriot, for he says that colors arise only at places where the refraction is a maximum, i.e., where the angle at the drop between the incident ray and the visual ray is 135°.[21] In a work defending astrology,[22] published in 1610, Kepler again asserted that the colors of the rainbow are due to solar rays passing through round droplets of rain; but he did not explain his views further.

Most assuredly one would have expected Kepler to go into further detail in his second classic treatise on optics, the *Dioptrice* of 1611. Nevertheless, one finds here only the familiar statement that the colors of the rainbow arise where refraction is great.[23] In the preface Kepler said that he had thought of adding a little book on the rainbow but that satisfactory causes of parhelia, which are also the causes of extraordinary rainbows, were to be desired, and these at present have failed to appear.[24] Kepler apparently never did write this projected book, even though he seems to have maintained confidence in his ultimate explanation. In correspondence with Jo. Remus in 1619 he declared that certain things concerning the rainbow and halo are clear. The radius of the primary bow is 45°, that of the secondary being 11° greater; and the radius of the halo is $22\frac{1}{2}°$. The reason is to be sought in the maximum refraction in the round drops of water, for where the refraction is greatest, there do colors arise. The same explanation is given by Kepler in notes accompanying his translation from Greek into Latin of Plutarch's *De facie in orbe lunae.* Here he says that Aristotelians too readily conceded that the secondary bow is a mirror image, upon an outer cloud, of the primary bow which appears on an inner cloud; yet Kepler's failure to explain the secondary rainbow is a serious deficiency in his own work.

Kepler and Harriot were not the only men of their time to explain the rainbow in terms of refractions and reflections in individual drops. In 1611, the very year of Kepler's *Dioptrice,* there appeared two other treatises on vision, light, and the rainbow—the *Photismi de lumine*[25] by Francisco Maurolico (1494–1575), Abbot of Messina, and *De radiis visus et lucis in vitris perspectivis et iride tractatus,* by Marco Antonio de Dominis (1566–1624), Archbishop of Spalatro. The former work includes three books on refraction, of which the second, devoted to the rainbow, was completed in 1553. It explains the rainbow in terms of internal reflections, forming within the drops octagonal star polygons, without recourse to refraction. The work of De Dominis, composed also a score of years or so before its publication, includes a short section on the best known pre-

[18] *Opera,* 2, pp. 72–73; *Epistolae,* pp. 378–379.
[19] Kepler, *Opera,* 2, pp. 73–74; *Epistolae,* pp. 379–380.
[20] *Opera,* 2, pp. 75–76; *Epistolae,* pp. 380–382.
[21] *Opera,* 2, p. 100. See also Brocard, reference 6, pp. 379–380.
[22] *Tertius interveniens. Das ist, Warnung an etliche Theologos, Medicos und Philosophos Dass sie nicht das Kindt mit dem Badt ausschütten* (Frankfurt a. M., 1610). In *Opera* 1, pp. 547–651. See especially p. 570.

[23] *Dioptrice,* 1, 26; *Opera* 2, p. 530.
[24] *Opera,* 2, pp. 524, 574.
[25] A portion of this work had appeared earlier at Venice in 1575, but this edition is rare. An English translation from the 1611 edition, made by Henry Crew, was published at New York in 1940.

Cartesian explanation of the rainbow.[26] So closely does the theory of De Dominis resemble, qualitatively, the account in the *Les météores* of 1637 that Leibniz and Newton, apparently quite unfairly, virtually accused Descartes of plagiarism. However, the explanation which De Dominis gave of the primary bow is incomplete, since he overlooked the fact that the rays must be refracted on emerging from the raindrops, as well as upon entering; and his account of the secondary bow is entirely mistaken.[27] Moreover, De Dominis did not account quantitatively for the size of the bows; and hence it is not clear why his explanation should be preferred to that of Kepler.[28] After all, as Maurolico said, nearly the whole of the demonstration depends upon the apparent size of the bow.[29] To Kepler one owes the clear recognition that "to measure is to know;" and to him physics appears to be indebted for the earliest *quantitative* theory of the rainbow based upon refraction in raindrops. Had he but measured more accurately, he might have anticipated the theory that Descartes gave half a dozen years after Kepler's death.

Snell, in a work on the comet of 1618, referred to haloes and rainbows as caused by reflection and refraction, promising that on another occasion he would comment more fully on this idea; but his commentary, if written, is not extant. One wonders if it might not have served as a connecting link between De Dominis, with whose work Snell apparently was familiar, and Descartes. Kepler, however, seems not to have been aware of the works of Maurolico and De Dominis, for he never referred to their theories of the rainbow. There appears to be but a slim chance that Descartes was directly indebted to Kepler, for the explanation sent to Harriot in 1606 was not published until 1718.[30] However, it is not unlikely that the efforts of Kepler were known to scholars in Germany. Years later, in 1653, G. A. Kinner von Löwenthurn, tutor in the household of the emperor Leopold I, wrote to Huygens from Prague that the explanation of Descartes did not satisfy him.[31] He referred instead to works on the rainbow writted by two Jesuits, Balthasar Conrad (1599–1660) and Johannes Marcus Marci (1595–1667). These men had spent many years in Prague, where Kepler had lived while he was studying the rainbow, and Kepler's relations with the Jesuits had been very friendly. Kinner says that many years before, in connection with a demonstration which Conrad had proposed and Marci had attacked,[32] the question had arisen as to whether rays which strike the spherical drops tangentially are refracted in the same manner as those which travel along secant lines.[33] It would appear from this query that the theory of Kepler did indeed live on—in spirit if not in fact—for half a century or more; and it is not impossible that its influence was felt indirectly by Descartes. One thing at least is certain, and that is that the Cartesian explanation was not an exception to the rule that scientific theories do not come as unanticipated bolts from the blue.

[26] The work was revised by the author before publication, and an account of the telescope, invented shortly before, was added.

[27] For a critical summary of this work see R. E. Ockenden, "Marco Antonio de Dominis and his explanation of the rainbow," *Isis* **26**, 40–49 (1936).

[28] Certainly it is misleading to say [as does H. E. White, *Modern college physics* (New York, 1948), p. 401] that "The elementary theory of the rainbow was first given by Antonius de Demini [sic] in the year 1611 and later developed more exactly by Descartes."

[29] See *Photismi de lumine* (Crew translation), p. 103.

[30] See Kepler's *Epistolae*, reference 15. Leibniz, however, called attention to a remark by Huygens that the basis of what Descartes had given on the rainbow "has been taken from a place of the incomparable Kepler." See Leibniz, *Opera omnia*, vol. V (Genevae, 1768), p. 547.

[31] See Christiaan Huygens, *Oeuvres complètes* (19 vols, La Haye, 1888–1937), **1**, p. 239.

[32] Conrad wrote *Propositiones physico-mathematicae de flamma iride* (Olomutii, 1634); Marci published two works on the rainbow, *Thaumantias, sive liber de arcu caelesti dequecolorum apparentium natura, ortu et causis* (Pragae, 1648), and *Dissertatio physica curiosa in propositiones mathematicas de natura iridis* (Pragae, 1650). I have seen only Marci's *Thaumantias*.

[33] Huygens replied that tangential rays are indeed refracted as are other rays, but he expressed confidence in the Cartesian theory nevertheless.

But for the scientist it is not only honorable to doubt, it is mandatory to do that when there appears to be evidence in support of the doubt.—Robert Oppenheimer, "Encouragement of Science," *Science* **111**, 373–375 (1950).

The Caloric Theory of Heat*

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MANY institutions offer advanced undergraduate courses in physics which are designed to introduce students to the methods of research. The physics department of the Massachusetts Institute of Technology has such a course which is primarily a laboratory course. Much of the preparation time is taken up by a two-weeks' home quiz which introduces the students to the problem of evaluating experiments, or theories based directly on experiment. The use of historical scientific material allows the student not only to think seriously about the validity of the concepts he has been taught but also gives him a problem to which he cannot find the answer worked out for him.

Because of the paucity of relatively concise statements of such historical material it is generally necessary for the instructor to prepare such statements for himself. The following paper is an illustration of such a write-up which has proved very useful as an exercise in evaluation of experiments and theory. Using such material, the student can be asked to criticize the experiments presented and suggest in considerable detail other experiments which would seem to him more convincing. In the particular problem of the caloric theory of heat, the students were asked to present their scientific reasons for believing the energy theory of heat.

The Caloric Theory

The fundamental assumption of the caloric theory was the concept that heat consisted of a fluid capable of penetrating all space and able to flow in and out of all substances. This fluid was called caloric. The important principles of its action were that it was self-repulsive and was strongly attracted by matter.

Matter was considered to be made up of atoms consisting of discrete particles attracted toward each other by their mutual gravitational attraction. If the gravitational attraction were the only force which occurred, every particle of matter would be attracted toward each other, resulting in a single solid homogeneous mass. To prevent this conclusion, a repulsive force was postulated which was considered to be the self-repulsive caloric.

Thermal Expansion

The caloric theory supplied an obvious solution to the problem of thermal expansion and contraction. Heating a body consisted of adding the fluid caloric to the body and it expanded. On cooling, the caloric fluid was removed and hence the body contracted. The detailed behavior of many of the phenomena of heat was explained by considering[1] each atom to be surrounded by an atmosphere of caloric whose density diminished more rapidly than the intensity of the gravitational attraction with distance from the center of any particle.

The gravitational attraction was considered to be inversely proportional to the square of the distance from the center of the atom while the caloric atmosphere which caused the repulsion was assumed to obey a logarithmic law by analogy to the earth's atmosphere. This might be illustrated as shown in Fig. 1, where the gravitational attraction due to the atom m is represented as a solid line and the caloric repulsion as dotted. At the point where they are equal, point P, another similar particle would be at equilibrium. If the temperature of the body of which m is an atom was increased, the caloric atmosphere around each atom increased and the caloric curve of Fig. 1 was thought of as rising. When this occurred, point P receded from the center of m, corresponding to the expansion of the body. According to this picture, if the temperature of the body was increased in uniform steps, the height of the caloric atmosphere increased in a uniform fashion. This is illustrated in Fig. 2. The theory predicted that the dilation of a body should not be a uniform function of the temperature but should increase with increasing temperature as

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[1] Emmett, *Annals of Philosophy* 9, 421 (1817).

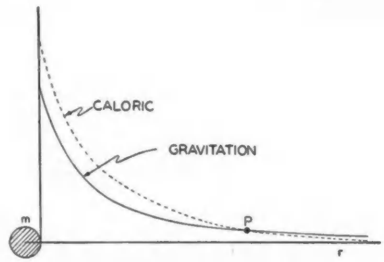

FIG. 1. Caloric repulsion and gravitational attraction between atoms. The gravitational attraction due to the atom m is represented by a solid line, and the caloric repulsion by a dotted one. At the point P, where they are equal, another particle similar to m would be in equilibrium.

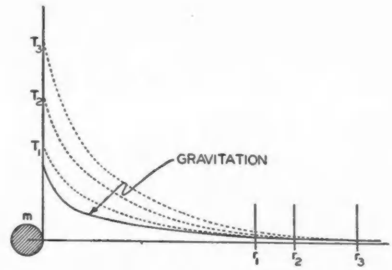

FIG. 2. Representation of the increase in dimensions of the caloric atmosphere with temperature. For uniform emperature steps, the increase of height is uniform.

was shown to be the case by the experiments of Dulong and Petit.[2]

The theoretical difference between solids, liquids, and gases was considered to lie in the degree of gravitational attraction between the atoms of the substance. With small amounts of heat, the caloric repulsion was not strong and the atoms were considered tightly bound by a strong gravitational attraction. As the temperature of the body was increased, the attraction became less as the repulsion became greater. In a liquid, the caloric content was sufficiently high so that the atoms were not held in a rigid position by the mutual gravitational attraction. In a gas the gravitational attraction was considered to be negligible. Thus the observed effect was predicted that the expansion of a gas would be much greater than in the case of a liquid, which in turn would be greater than that of a solid. The fact that the expansion coefficient increased with temperature much more rapidly for liquids than for solids, which was a result of this theory, had been demonstrated by the experiments of Lavoisier and Laplace.[3]

Proponents of this theory argued that the negligible effect of the gravitational attraction between atoms of a gas should be apparent in at least two ways. Since the gravitational effect depended upon the mass of the atoms, it was expected that solids and liquids differing in the mass of their atoms should have different expansion coefficients. Since for gases, the gravitational attraction was considered negligible, all gases should have the same thermal expansion coefficient. Also, the expansion of a gas per degree change in temperature should be independent of the actual temperature. Both of these effects had been found to be true for gases following the experiments of Gay-Lussac.[4]

Although all substances were expected to expand on the addition of caloric, and contract when the caloric was removed, the exception to this rule for the case of water was known. The experiment of Count Rumford[5] on this point had proved beyond doubt that water passed through a point of maximum density at temperatures around 41°F. No reason was advanced for this exception but it was not considered too remarkable since other similar cases of fluid mixtures were known. Mixtures of water and alcohol, or molten copper in tin were both known to be accompanied by a decrease in volume.

Specific Heat

A careful distinction was drawn between the intensity of heat and the quantity of heat. Figure 3 shows a graph representing the density of the caloric atmosphere around an atom, as a function of the distance from the atom. The two curves represent the caloric density at two different temperatures. The intensity of heat is represented by the temperature and therefore by the actual density of caloric at the surface of the atom. All atoms did not have identical calorific atmospheres, and although they all had a logarithmic dependence of caloric density on distance, the rate at which the atmospheric density falls

[2] Dulong and Petit, *Ann. de Chem. et de Physique* **7**, 113 (1818).

[3] Biot, *Traité de Physique* Vol. I, p. 158 (1816).

[4] Gay-Lussac, *Ann. de Chem.* **43**, 137 (1802).

[5] Rumford, *Nicholson's Journal* **11**, 225 (1805).

off, varies from substance to substance. The quantity of heat required to change a body from temperature T_1 to temperature T_2 is represented by the shaded difference between the two curves of Fig. 3. Since these curves were considered different for different types of atoms, the quantity of caloric involved in the same change in temperature for different types of material was expected to be different. This quantity of heat fluid required to produce a given change of temperature for a given amount of material was called the specific heat by analogy to the term specific gravity.[6]

The experiments of Dulong and Petit[7] showed that the product of the specific heat of each solid element by the weight of its atom gave a constant value. This led to the conclusion that the amount of caloric surrounding an atom was related to its atomic weight in a manner which was expected from the attraction between matter and caloric. In 1780 Laplace and Lavoisier showed that the specific heat of a substance was not a constant, but increased with temperature. This agreed with the caloric theory. With reference to Fig. 3, as the temperature increased in uniform increments, the difference in area between the successive caloric density curves increases, which predicted greater quantities of heat necessary for the change to take place, as was observed.

Changes of State

At a sufficiently low temperature, all known substances were solid. As the temperature was raised, caloric was attracted toward the atoms of the substance and eventually the substance liquefied. If more caloric was added, it would take the gaseous form. Black[8] showed that when a change of state occurred, the heat added to the body went only into the change in phase and the temperature of the body remained constant during the process. To explain this he introduced the concept of latent heat.

According to Black's theory, heat could take two different forms, sensible and latent. Changes in sensible heat corresponded to those changes which could be measured by changes in temperature. Besides this effect of the attraction between matter and caloric, in which the caloric merely forms an atmosphere around the atoms and molecules, caloric was considered actually to combine with an atom in a fashion similar to the chemical combinations of the atoms themselves. When this occurred, the caloric lost its sensible form and became latent. The "chemical" combination of an atom with caloric produced a new compound in which neither the original atom nor the caloric retained its identity. It took place, as did ordinary chemical reactions, only in definite proportions and under definite circumstances. No heat was considered lost in the process, since it was reversible; cooling a body down returned the caloric back to its sensible form.

No new explanation was necessary to describe the transition from liquid to gas. For the same substance, there seemed to be no particular relation between the latent heat of fusion and the latent heat of vaporization, even though the same process was active in both. The case of vaporization, however, seemed to give an additional check on the validity of the caloric theory. The theory postulated that the requirement for a reaction to take place between the sensible caloric and an atom was a critical amount of caloric around each atom. Since caloric was self-repulsive, the temperature at which vaporization took place ought to be changed by changing the external pressure. When a piece of iron was compressed by hammering, sensible caloric was squeezed out and the surface of the iron became hot, or if gas was compressed it emitted caloric and became hotter. It seemed amply proved, therefore, that sensible caloric could be squeezed from a body by artificially pushing the atoms into a closer proximity than the mutual repulsion of their caloric atmospheres would allow. Thus if pressure were put on a substance near its boiling point, some of the

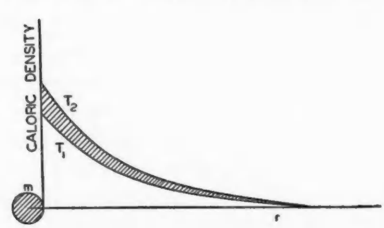

FIG. 3. The shaded area represents the quantity of heat required to change a body from temperature T_1 to temperature T_2.

[6] Wilcke *Proc. Roy. Soc. of Stockholm* (1772).
[7] Dulong and Petit, *Ann. de Chem. et de Physique* **10**, 395 (1819).
[8] Black, *Lectures on the Elements of Chemistry* (1803).

sensible caloric would be lost from the substance and a higher temperature would have to be applied before sufficient caloric was available to the atom for a vaporization reaction to occur. This fact had been amply borne out by experiment.

The Communication of Heat

Conduction was a very obvious method of transferring heat from one place to another because of the very great attraction between matter and caloric. The less caloric a body had, the greater the attraction of its atoms for the caloric fluid. In adding heat to one end of a solid bar, the atoms at the heated end acquired more caloric than their neighbors, and by having more, their attraction for this caloric was less. The neighboring atoms attracted the caloric away, and continued to do so until all the atoms of the substance had achieved the same caloric atmospheres. The facility with which caloric could be passed from one atom to another depended upon the structure and composition of the substance. In apparent agreement with the caloric theory, Count Rumford[9] found that even in the same substance, the conducting power increased with the compactness of the structure.

Count Rumford, who was one of the most famous opponents of the caloric theory, was the first to prove, in 1785, that heat could be transmitted through a vacuum. This is particularly interesting because the fact that heat could be transmitted through a vacuum without the aid of a material medium seemed to be one of the great stumbling blocks in the theory which described the nature of heat as a wave motion. It was a natural consequence of the caloric theory. Since caloric was a self-repulsive fluid which pervaded all space, it was to be expected that where there was no matter to impede its progress, it would expand without limit.

The radiation of heat was known to be strongly influenced by the nature and condition of the radiating surfaces. A perfectly polished surface was considered to have all the molecules lying in a plane, and all the molecules as close together as possible. Thus, since the greatest amount of mass was to be found at this kind of surface, the caloric was most tightly bound, and could be lost only with the greatest difficulty. This defined a poor radiator. That it was a good reflector would be predicted since there already existed a tightly bound caloric surface layer; this would repel any further caloric coming toward the surface. The good radiators were rough surfaces, which meant that for a given area of surface, much less mass lay in a plane and hence the caloric was held only by a few groups of atoms at any one place. The neighboring atoms did not have the cumulative effect present if there were more of them as neighbors, so the caloric could escape more easily and one obtained a good radiator.

Heat from Mechanical Work

When gas was put in a cylinder and compressed, heat was given off. The atoms of the gas were considered unaffected by the gravitational attraction of neighboring gas atoms, and held apart by the caloric atmospheres surrounding each atom. Pushing the atoms closer together could only be done by overcoming the thermorepulsion by mechanical force. In so doing, caloric was squeezed out of the gas and it gave off heat. If the reverse experiment was tried and the gas expanded, the temperature would fall since the atoms would be farther apart than their caloric atmospheres would keep them.

The removal of heat by compression was by no means confined to gaseous bodies. The action of compression on solid bodies afforded a further demonstration of the existence of the caloric fluid. It was noticed especially that caloric was disengaged by pressure or percussion only so long as bodies underwent condensation. A common experiment was to show that a piece of iron made red hot by hammering could not be strongly heated a second time unless it was reheated in the fire. This fact was explained by supposing that the fluid heat which had been pressed out of it by percussion was recovered in the fire.

Among the most famous experiments conducted for the purpose of disproving the materiality of heat, were those of Count Rumford on the "Source of Heat which is Excited by Friction."[10] He showed that a large amount of heat was produced in the process of boring cannon, and he could find no other source of heat in his experiment except that caused by friction.

[9] Rumford, *Phil. Trans. Roy. Soc.* (*London*) **77**, 48 (1792).

[10] Rumford, *Phil. Trans. Roy. Soc.* (*London*) **88**, 80 (1798).

Rumford concludes his paper with the following statement: "In reasoning upon this subject, we must not forget to consider the most remarkable circumstance, that the source of heat generated by friction in these experiments appears to be inexhaustible. It is hardly necessary to add, that anything which an insulated body or system of bodies can continue to furnish without limitation, cannot possibly be a material substance; and it appears to me to be extremely difficult, if not impossible, to form any distinct idea of anything capable of being excited and communicated in the manner heat was excited and communicated in these experiments, except it be motion."

Count Rumford realized that his experiments would not go unchallenged. In fact he raises a number of questions in order to answer them himself. "Is the heat furnished by the metallic chips which are separated by the borer from the solid mass of metal? If this be the case, then, according to the doctrines of latent heat and of caloric, the capacity for heat of the parts so reduced to chips ought not only be changed, but the change undergone by them should be sufficient to account for all the heat produced, but the capacity is not changed, [a fact the Count proved in the course of his experiment]. From hence it is evident that the heat produced could not possibly have been furnished at the expense of the latent heat of the chips." The answer to this question from the point of view of the caloric theory was presented by Emmett in the following way.[11] "In the commencement of this reasoning, an assumption is made, which is particularly unfortunate: namely, that if heat being an elastic fluid be evolved by the compression of solid matter, the capacity of that solid for heat must be diminished in proportion to the quantity which has been separated. The whole quantity of heat contained in the solid is doubtless diminished, but why is the capacity to be changed? . . . That the quantity of heat evolved in this experiment was great cannot be disputed, yet it was by no means sufficient to warrant the conclusions that have been drawn. . . . In these experiments, a very large mass of metal was submitted to an excessive pressure, and of the mass, fresh strata was continually exposed to the compression by the wearing off of the brass: hence a definite quantity of heat was separated from each stratum in succession. Now if we admit the existence of caloric in a state of great density in the metals, this cause would be quite adequate to the production of the observed effect. The greatest error appears to be the assumption that the source of the heat thus generated is inexhaustible; the quantity that can be thus excited is finite" but will not cease, according to this picture, until all the brass is worn away.

Let us return to more of Rumford's questions. "From whence came the Heat which was continually given off in this manner in the foregoing experiments? Was it furnished by the air, was it furnished by the water which surrounded the machinery? Is it possible that the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? Or the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions appear most improbable." The answer to these questions seemed to lie at the very foundation of the caloric theory, namely that caloric pervaded all matter, and therefore could be furnished by all those sources which the Count appeared to discredit. His whole apparatus was bathed in an atmosphere of caloric. A physical picture of the manner in which the caloric was resupplied to the brass cup during the experiment was described as follows by a correspondent to the *Philosophical Magazine* for 1816:[12]

"If heat be a material fluid, the effect of force on a body containing it would be similar to the force on a body containing any other fluid diffused through its pores in a similar manner. Water being a fluid which in many instances produces effects similar to those produced by heat, it appears best adapted to illustrate the generation of heat by friction.

"I procured a piece of light and porous wood . . . and having immersed it in water until it was saturated, I fixed it firmly over a vessel filled with water, the lower end being . . . below the surface of the water, and then moved a piece of hard wood backwards and forwards on the upper end, with a considerable degree of pressure. I thus found that water could be raised through the pores of wood by friction. The process is easily understood: the piece of hard wood, as it is moved

[11] Emmett, *Annals of Philosophy* **16**, 137 (1820).

[12] *Phil. Mag.* **48**, 29 (1816).

along, presses the water out of the pores, and closes them, driving out the water which is pressed out before it; but when the hard wood has pressed over these pores, the water from below rushes into them to restore the equilibrium.

"The action of the blunt borer in Count Rumford's experiments appears to have produced a similar kind of effect, the heat having been forced out of the pores of the metal by the borer, its place would be supplied by heat from the adjacent parts. Gun-metal being a good conductor, the neck which connected the cylinder with the cannon would be capable of giving passage to all the heat which was accumulated from the cannon, and the other conductors with which it was connected."

Besides the friction experiments of Count Rumford, the opponents of the caloric theory championed a somewhat similar experiment by Davy.[13] Davy showed that ice could be melted by rubbing two pieces together in a vacuum, the whole apparatus being at the temperature of freezing water. Rumford showed that heat was transmitted through a vacuum, so that there was no barrier to a supply of caloric. Furthermore it had been demonstrated that when pressure was applied to ice, its melting point fell below 32°F. Friction cannot occur between surfaces without the application of pressure, the ice blocks kept the temperature of the surface at 32°F which became higher than the melting point in the process of the experiment, and it appeared to be perfectly proper for the ice to melt without recourse to any theory as to the nature of heat.

The Weight of Caloric

The explanation of many caloric phenomena was based on the strong attraction between caloric and matter. Because of this attraction, it was expected that the force between the caloric in the body and the earth, namely the weight of caloric, should be a measurable quantity. One of Count Rumford's major investigations was an attempt to measure this weight of heat.[14] His experiments consisted in part of the following. He constructed two identical flasks, one containing water and the other containing mercury. These he sealed off and weighed with great care at 61°F and 30°F. Since the specific heats of the two substances were very different, and particularly since the water froze and therefore gave up a large quantity of latent heat, Rumford felt that having observed no change in weight "all attempts to discover any effect of heat upon the apparent weights of bodies will be fruitless."

Many of the caloricists did not believe that this was a conclusive experiment. The type of objections they brought forth was similar to the following discussion of the editor of the *Philosophical Magazine*.[15]

"In the various direct attempts that have been made to weigh heat, I fear philosophers have been following a plan just about as rational as it would be in the inhabitants of the ocean to attempt to weigh water by employing a balance suspended in the medium that surrounds them, and putting into one shell a substance that to them should seem wet, and into the other a substance which they might call dry!

"If I suspend a piece of metal in water at one end of a balance, and if to this metal I join a bit of cork, will not the mass, by having its absolute gravity increased, have become specifically lighter? In this case no one hesitates in admitting absolute gravity is the cause of the compound body appearing lighter when weighed in water, a medium more rare than one of the ingredients, but denser than the other, but if heat instead of cork had been added to the metal would not the effect have been the same, an increase of volume and a diminution of specific gravity? And for the same reason too, the metal being heavier but the heat lighter than water.

"Now in the case before stated, why has the increase of absolute weight not been observed? I take the reason to be this: Rumford attempted to determine it in the air; overlooking this plain fact, namely, that air may be considered as bearing the same relation to heat that water does to any substance many times lighter; that is, the air, though a rarer substance than the solid bodies weighed in it, is a denser one than heat."

Discussion

The caloric theory has a great influence on our teaching of the elementary concepts of heat. Much of the nomenclature in common use is

[13] Davy, *Nicholson's Journal* **4**, 395 (1799).

[14] Rumford, *Phil. Mag.* **5**, 162 (1799).

[15] Tillock, *Phil. Mag.* **9**, 158 (1801).

based on the older theory: we talk about heat flowing from one place to another, and many text books discuss the concepts of specific heat and calorimetry essentially in terms of the caloric theory. The terms specific and latent heat had a descriptive meaning in the heat fluid theory, and the unit, and in fact the use of the word quantity of heat, are caloric theory concepts. A careful study into the details of what is being taught at the elementary level concerning the behavior and phenomena of heat shows that to a large degree students are introduced to the subject by way of the caloric theory of heat and it is not until the more advanced courses that a real effort is made to present a consistent discussion of heat as a form of energy.

Do Students Find History Interesting in Physical Science Courses?*

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THIS symposium is not only evidence for, but a consequence of, a reviving interest in the rich heritage which we possess in the field of physics. The reports we have just heard[1] are examples of the efforts that many individuals are making to bring to light and interpret the documents so essential for this purpose. We are indebted not only to them but to the officers of this Association and the editors of the *American Journal of Physics* for so freely publishing this material. Few people would disagree that for a scholar in *any* field, a clear grasp of its historical development is of great value, whether he is doing research to extend its frontiers or interpreting it to the younger generation in teaching.

With all this I am in hearty accord. And yet I have misgivings—and I can sense the doubts in your minds—about the wisdom of the next step. If history is good for us, and in fact fascinating when presented as we have heard it this morning, shouldn't we in turn introduce more of it into our teaching? Why do we hesitate? It may be merely that we feel there is insufficient time in our already crowded courses. But I think that is more of an alibi; there are deeper reasons than that. We hesitate because we believe the students have little interest in the queer ideas and fumbling efforts of the past. They want to know the very latest developments, the newest wrinkles. You and I know how keen they have been to learn more about the recent wartime application of physics. Today it is workings of the hydrogen bomb which has the greatest appeal.

Let me ask you to recall for a moment your undergraduate days and the physics courses you took. Nearly all of us, I believe, heard the caloric theory of heat at least mentioned. What was our general feeling at the time? Wasn't it one of pity for those misguided fellows who bored cannons or stirred water to try to prove something that was quite obvious to us? Did we find any fascination in a scientific debate that lasted nearly fifty years? Or in our chemistry courses, what attitude did we take toward that mysterious stuff, phlogiston? Even in graduate school, I noticed that most of the students were rather annoyed when they were told to expect some questions on the history of physics in their preliminary orals.

It is not hard for me to understand the causes of this antipathy toward history of science on the part of many students. One reason is that the historical references they hear or see are fragmentary. In their textbooks these references frequently appear in small type or as footnotes and the students readily conclude that they are introduced mainly for decoration. If they read many quotations from original sources they are troubled with strange terms and concepts. "Vis viva" is translated as "living force" but they are still mystified about what it represents. "Galvanism" sounds like a religious sect, while "elastic fluid" may suggest liquid rubber more readily than it does a gas. But behind all these rather trivial

* The fourth paper of a Symposium "Use of Historical Material in Elementary and Advanced Instruction," presented during the annual meeting of the American Association of Physics Teachers, Barnard College, New York, February 4, 1950.

[1] See the three preceding articles by I. Bernard Cohen, Carl B. Boyer, and S. C. Brown in this issue.

matters, I think, lies the basic reason that the excursions which the student takes into history are so disconnected and brief that he has no opportunity to make the difficult transition from his present day point of view to that of the early worker. Unless that transition is achieved, he will benefit little from the effort, although he may possibly remember that Joseph Henry used his wife's silk petticoats for electrical insulation or that Thomas Young (besides deciphering the Rosetta stone) played the flute, sang and danced!

These opinions of mine are subjective impressions and it is quite possible that I have taken an extreme view. With this in mind I recently submitted a questionary to those students now taking the general physics course at Colgate. Their lack of interest in historical materials is not as complete as I assumed but it is still definite. Of the idea that some outside readings in history be suggested, they are three-to-one in favor, but for outside readings in history to be *required*, they are ten-to-one opposed. Only half of them are willing to indicate that they do not like the history of science and see little point in including it in a science course. But 75 percent of them claim that more would be lost than gained if some topics were omitted, and that time were devoted to developing the historical background of others.

Before I appear to apply a sweeping indictment to all elementary college physics courses for their failure to interest students in the history of the subject, let me acknowledge that there are notable exceptions due primarily to exceptional people. The late Lloyd W. Taylor of Oberlin College was perhaps the most outstanding example of those physics teachers who believe that the historical approach is a valuable means of presenting the subject. In teaching his course, as his well-known text, "Physics, the Pioneer Science"[2] shows, he wove into the very fabric of his instruction the roots of each topic and the struggles of men's minds to discover our present answers. From all I have been able to learn, his approach was successful and was liked by most of his students. There are doubtless many other courses following a similar plan.

But this problem of using history most effectively, and awakening the students' interest in it, can be dealt with in more than one way. One of the general education courses at Harvard, initiated by President Conant, uses the case-history method where the entire year is devoted to treating four or five cases,[3] one of which, for example, is the seventeenth century development of pneumatics with special reference to the work of Robert Boyle. The reports I have heard seem to show that this course is quite successful in illustrating the "tactics and strategy" of science, and I rather doubt that there is any widespread antipathy toward the historical material on the part of the students who elect it.

I am more familiar, however, with the operation of the general education course in physical science which we are now teaching at Colgate University for the fourth year. This is a one-semester course which is paired with a similar course in the biological sciences and required of all freshmen. In planning the course it was our earnest ambition to draw the students' natural interest in the factual results of science over into a study of how these results are obtained. Along with this objective, we were anxious to teach the students to use methods of reasoning and critical thinking instead of mere memorization of authoritarian material, as so many of them are trained to do in secondary schools. So we decided to organize the course around a group of problems selected from the various areas of the physical sciences. The student is not expected to solve a problem by himself, but after suitable background reading he participates in classroom discussion where a solution is attempted under the guidance of the instructor.

An important difference between our course and Conant's course is in the use of history. One of our problems, "What is the Significance of the Periodic Table?", is treated as a case-history much as the Harvard cases are handled. But in most of our problems the history is not introduced at the beginning and it may not be used at all. For example, our first problem raises the question, "Does the earth travel around the sun, or the sun around the earth?" This problem could be approached by starting back with Pythagoras and the other Greek scholars showing the rise and fall

[2] Taylor, *Physics, the Pioneer Science* (Houghton Mifflin Company, Boston, Massachusetts, 1941).

[3] Publication of the first two cases by the Harvard University Press has recently been announced.

of a number of systems for explaining the celestial motions, leading up to the final solution which stems from Newton's law of gravitation. But this is a required course for all students, and we were concerned with the matter of motivation. We thought it wiser on the whole to start where the students were, and lead them later into other historical epochs when they were better prepared.

Briefly, the way we treat this problem of celestial motions is to persuade the students first to agree to a set of ground rules, namely, that they are entitled to read and make use of any observations they find reported by other authorities, but that the process of interpretation, drawing inferences, and forming hypotheses will be carried on in class discussion. Then one step at a time we take up the most obvious motions, the diurnal ones, which we account for by a rotation of the earth on its axis. The daily eastward slip of the moon in front of the stars can be explained by assuming it to revolve in an orbit about the earth with a period slightly over 27 days. The instructor then suggests by inference that the eastward slip of the sun at one degree per day could likewise be accounted for by the sun's revolution about the earth once per year. When the planets are studied, it turns out that their behavior can be explained satisfactorily by assuming that they revolve about the sun which at the same time carries them about the earth. (This, incidentally, was Tycho Brahe's model, except for the rotation of the earth.) In short, all the positional observations in the sky can be satisfied by a hypothesis which the students firmly believed up to that point to be wrong!

We naturally do not terminate the problem there. Additional investigations need to be made of the relative distances and sizes of some of these celestial bodies, and this we do. Their motions need to be consistent with the assumed laws of dynamics, and we rather soon find our initial hypothesis inadequate. We then try the heliocentric system as an alternative hypothesis. It turns out, of course, that this system can explain the positional observations with equal success and, furthermore, it has no trouble in meeting the dynamical requirements.

It is at this stage, after the students have wrestled with the problem for two weeks, that we assign some readings which take them back into its history.[4] They soon develop a genuine respect for the early efforts of the Greek scholars, they admire Aristarchus' insight, and at the same time admit that his heliocentric proposal would have had little or no chance of being accepted by his contemporaries. When they study Tycho Brahe they can see how he used his inability to observe stellar parallax as a deductive test to disprove the Copernican view and to support what he intuitively preferred, a geocentric system. The students also realize that Galileo's telescopic observations may have cast doubt upon the old geocentric system but they did not provide positive proof of the heliocentric. Newton's theoretical work then comes in its proper setting, and the success of the predictions of the existence of the planets Neptune and Pluto during the last century furnishes the final confirmation in the minds of educated men.

The acquaintance they gained with the solar system in the first problem makes it appropriate to raise questions as to its origin. This forms our second problem, and we treat this one in direct historical fashion, considering one hypothesis after another. Each raises new hopes, and all except the most recent seem to have some fatal defect. In fact, last October we were obtaining our source material directly from the daily newspapers as new hypotheses were proposed by Kuiper,[5] Struve[6] and Urey![7] This problem permits us to take the students to the forefront of scientific investigation, which is not possible in many fields. For two years the students have rated this problem as the most interesting of the course, surpassing the highly popular "atomic energy" problem.

The caloric theory of heat turns up in our course also, not as a topic by itself but in another connection. The problem is, "Why does the atmosphere grow colder at higher altitudes?" After the evidence is discussed and the relations of pressure with depth in a liquid considered, the

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[4] For example: George deSantillana, "Greek Astronomy," *Scientific American*, April 1949, p. 44.

[5] G. P. Kuiper, *New York Times*, Oct. 10, 1949; *Science News Letter* **56**, 259 (1949).

[6] Otto Struve, "Double stars," *Scientific American*, Oct. 1949, p. 42.

[7] H. C. Urey, *New York Times*, Oct. 27, 1949; *Science* **110**, 445 (1949).

students begin to realize that the atmosphere has different properties from the ocean because of its easy compressibility. When further discussion, supported by evidence, brings out the existence of convection in the atmosphere the students begin to realize that the cooling of a gas upon expansion is a natural property of the gas and can even be observed in the laboratory.[8] At this point they are asked to read portions of an 1832 text[9] describing the caloric theory of heat. Here they find an admirable (though qualitative) explanation for the adiabatic cooling and heating of a gas: when a gas is compressed the caloric it contains is confined to a smaller volume, the caloric density rises and hence the temperature rises. The reverse occurs when the gas expands. There is always some smart lad in the class who believes the caloric theory to be out of date, and this furnishes the point of departure for a study of its history. Thanks to some suggestions from Professor Sanborn Brown,[10] we are able to supply fairly good defenses for the caloric theory to answer Count Rumford's cannon boring experiment, and then students begin to realize a little better why the argument lasted several decades. When we get to Joule's work, we have an excellent opportunity to point out how quantitative measurements can frequently resolve an argument previously supported by qualitative observations.

It is interesting to note that while we were at first skeptical of historical material and used it sparingly, as the course has evolved during the past three years we have added increasing amounts. We were guided by the subjective opinion that the students did not *mind* the history and its use helped to pursue our objective of showing them how science really works. It was only recently that we obtained any data to support this view. The freshmen were asked to indicate their opinions on a questionary similar to the one given to the general physics students. Let me quote a few samples. With reference to the problem of the earth and the sun, they agree nine-to-one with the statement,

> The history of this problem was worth taking up in spite of the limited time available,

and ten-to-one that,

> The difficulties which faced the early thinkers when they tried to account for celestial motions can be appreciated much better after two weeks of struggling with the problem oneself.

In the atmosphere problem, however, they were less convinced. Their opinions were equally divided as to whether the introduction of the old caloric theory was confusing and made it more difficult to understand our modern theory of heat. Apparently there is still room for improvement in our treatment of the caloric theory. Any student left with a sense of confusion about a topic will dislike it and incline to some other method of dealing with it.

Their general attitude toward history was favorable, however, as is indicated by nearly three-to-one agreeing that,

> I like to get into the history of a scientific problem, because then I understand the problem better.

and four-and-one-half-to-one agreeing that,

> Some appreciation of the evolution of ideas is worth having, even at the expense of detailed study of modern theories.

As I think about this whole matter of the history of science there are a few generalizations I should like to make.

1. A study of the history of a problem gives one a broader understanding of the problem, a greater respect for the intellects of the earlier workers, and may create a better perspective for the scientific developments of today.

2. The historical approach is a valuable means for teaching in many areas. As Louis Pasteur[11] said nearly a century ago, the historical approach ". . . illuminates the intelligence. It enlarges it, cultivates it, makes it capable of producing, and fashions it for new inventions. . . . [It] shows

[8] This is one of the set-ups which they can see and operate in a demonstration laboratory introduced into the course at this point.

[9] Tiberius Cavello, F.R.S., *The elements of natural or experimental philosophy*, 5th Amer. Ed., Philadelphia, 1832, p. 365.

[10] Amplified in his paper, "The caloric theory of heat," in this symposium.

[11] H. H. Ackerknecht, tr., "Pasteur's report on the usefulness of the historical method in teaching science," *Am. J. Physics* **16**, 244 (1948).

that nothing durable is obtained without manifold effort." I would be one of the last to claim that this method will yield equal success if universally applied, but it may be that a wider but judicious use of this approach will produce beneficial results both to our scientists and to the citizens of society who have to endure (or benefit from) the results of science.

3. History interesting to us as experienced physicists is not necessarily, and perhaps not often, interesting to the student. The evolution of a problem, when followed by a teacher possessed of a broad background in the field and a fair degree of maturity, can be fascinating, but it is likely to leave a student completely cold. In the use of historical material, as in probably all the techniques of teaching, it is essential for the teacher to be fully sensitive to the student's point of view.

4. Perhaps related to the previous point, I believe that the history of a problem is always more interesting when one has grappled with the problem at some length oneself. This has certainly been borne out by our experience with our freshman physical science course, and may be the main reason why there is such a contrast between the attitude of these students and the attitude of the general physics students.

5. Students can be more effectively interested in the history of science if the teacher has a broad background in the subject himself. This may be a self-evident statement, but we also find support for it in studying the results of the questionary. The freshmen in the physical science course have one common weekly lecture together, common reading assignments, and common testing. Then they also meet three times weekly in sections of thirty or less for discussion and interpretation of their reading material. It is clear that those students, whose instructors have a greater background in the history of science, indicate a more favorable attitude toward the use of historical material. I might add that the instructor's background is still richer if he is at least aware of the times he is discussing. A teacher discussing Lavoisier's work, for example, who makes no reference to the French Revolution implies that science operates in a political and social vacuum. Nothing could be further from the truth.

What are the implications of these statements for training teachers who will handle our general education courses? There are many, and to go into them will carry us out of the topic of this symposium. I shall merely say that many of our instructors in this course at Colgate University have built their own background as they went along, or aphoristically, real education educates the educator as much as the student.

Lo! keen-eyed towering science,
As from tall peaks the modern overlooking,
Successive absolute fiats issuing.
—Walt Whitman, Song of the Universal.

The Physics-Mathematics Building at Michigan State College

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WHEN the Department of Physics and Astronomy vacated its old quarters in July, 1949 and removed to the new Physics-Mathematics Building a landmark on the Michigan State College campus passed out of use as a science-instructional center. The old structure, a portion of which dated from 1869, originally had been the home of the science departments of the institution, but since 1928 had been occupied with increasing inconvenience and congestion exclusively by the Department of Physics and Astronomy. Meanwhile, the enrollment at Michigan State College had increased from slightly more than 3000 in 1928 to more than 16,000 in the fall term, 1949. Physics and Astronomy class enrollments in the fall term, 1949, were 1322. The facilities of the old building were being seriously overtaxed to provide the bare minimum of classroom and laboratory space necessary for student instruction. Offices were crowded, and research laboratory space for the staff of 17 and for the nine graduate assistants was confined to the numerous nooks and crannies which the frequent additions and modifications to the original building had produced.

In 1946 the Michigan State Legislature authorized the State Board of Agriculture, controlling body for Michigan State College, to undertake the construction of a building to house the Departments of Physics and Astronomy and of Mathematics. A three-story-and-basement structure of reinforced concrete was designed by the architect, R. R. Calder, of Detroit, and constructed by the Reniger Construction Company, of Lansing, Michigan. A partial subbasement provided the space for the large amount of mechanical equipment needed for the operation of the building. After some delay caused by material shortages and the large volume of other construction on the Michigan State College campus, this building was occupied by the two departments in July, 1949.

FIG. 1. Exterior view of the new Physics-Mathematics Building at Michigan State College, East Lansing, Michigan. The view is of the front (east side) looking south.

The features of the building, except architectural, are not particularly new, but were adapted from those found in other similar buildings in various parts of the country by pooling ideas of the various department members who helped to decide on the final design in collaboration with the architect. No specific committee from among the department members was set up to assist in planning the building, but matters were usually decided by informal consultation among senior members of the department and with the engineers or architect.

The design of the Physics-Mathematics Building is modern and functional. The red brick exterior of the building is trimmed in limestone and is surrounded in front and rear with a parapet wall of cut stone separated from the building walls by a $6\frac{1}{2}$-ft dry moat, which gives the basement rooms full windows and excellent light. Throughout the building liberal use has been made of color in the decorative scheme—ranging from terra-cotta quarry tile on the window sills to two-color lecture room walls. In addition to being modern in every respect, the building is an attractive place in which to teach or study. Windows in most classrooms and laboratories extend from table height to the ceiling, a feature which gives to the front of the building the appearance of a huge sheet of glass. Figure 1 shows the front of the building before the grounds were landscaped.

The building measures 280 ft long and 64 ft

wide. A fan-shaped projection 69 ft by 93 ft on the basement and first floor provides space for the lecture rooms and for numerous laboratories. Classrooms, laboratories, and offices in the building are uniform in width (22 ft), but vary in length to suit the various needs. On all floors a corridor 13 ft wide provides access to the various rooms. Entrances to the front, ends, and rear of the building are arranged so that access is easy from any direction, and confusion in the corridors between classes is nonexistent. At all entrances of the building large plant boxes, an integral part of the structure, afford space for shrubbery to soften the outline of the building. Enclosed stairways and landings at each end of the building offer well-lighted and fireproof exits.

Around the aluminum-and-glass entrance doors sunken relief carvings tell the story of physics and mathematics to the visitor. Figure 2 shows the carvings around the main entrance. The sculptor, Mr. Carl L. Schmitz, has caught many of the crucial moments of science in his carvings. Great scientists are depicted in the acts of making some of the discoveries for which they are noted, while an occasional detail, such as is shown in the Galileo carving, Fig. 3, helps to suggest the times or the environment in which the scientist worked. Although it is to be expected that many of the scientists honored are of an earlier era, two living scientists are also portrayed in the group. Both Dr. Albert Einstein and Dr. E. O. Lawrence gave permission for including their likenesses in the sculptures.

The visitor entering the building from the front finds himself in a spacious foyer finished in green cinder block and primrose-colored ceramic tile. The terrazzo floor, likewise of a green hue, is divided by aluminum partition strips into a rectangular pattern. A self-operated elevator and broad stairs set off by hand rails of stainless steel offer the visitor access to all parts of the building. Lighted display cases set into the walls invite his attention to exhibits, photographs, and operating models of physical apparatus. On either side of the foyer wide corridors lead to offices, classrooms, and laboratories. Flush ceiling lamps light the corridors and an acoustically absorbent ceiling quiets any hubbub that might accompany the traffic along them. Floors of all corridors are covered in brown marbleized asphalt tile; walls of cinder block are finished green.

FIG. 2. Front entrance of the Physics-Mathematics Building showing the incised carvings. (Photograph by Mr. J. L. Beech.)

As shown in the floor plan, Fig. 4a, the first floor of the Physics-Mathematics Building is given over to the office of the Department of Physics and Astronomy, the two lecture rooms, seven elementary laboratories, storage rooms for the lecture demonstration and laboratory apparatus, and three staff offices. Here, as at many other institutions, the enrollment in the elementary physics courses constitutes a very large fraction of the total physics enrollment. On this floor are grouped the lecture rooms and the laboratories used primarily for instruction in the beginning courses. By grouping with the laboratories and the lecture rooms the two apparatus storage rooms, the problem of apparatus transfer is simplified. This is quite important, for example, when a lecture room must be used for several classes in a single day and when the lecture demonstration equipment must be removed and a new set-up substituted for the original equipment sometimes within an hour.

A lecture-recitation-laboratory system of instruction is used in the elementary physics courses at Michigan State College. Lecture sections, occasionally numbering as many as 150 students, are given theoretical explanations and demonstration experiments. For meeting these large groups two lecture rooms are provided on the first floor. The larger of these rooms seats 200 and the smaller 100. Seats in both rooms are tiered, those in the larger room being arranged in

FIG. 3. Detail of the Galileo carving at the main entrance of the Physics-Mathematics Building. (Photograph by Mr. J. L. Beech.)

circular rows and those in the smaller room in straight rows. Students generally enter the lecture rooms from the rear along ramps separated from the rooms by partition walls, making it possible for latecomers to enter with a minimum of confusion.

Both lecture rooms are windowless, the ventilation and heating being provided from louvres let in the walls. Recessed ceiling lights are used in the rooms. The walls are of cinder block painted ivory, but in the larger room the rear wall is finished a soft green like the hallways. Furnishings of both rooms are of birch; floors are of brown marbleized asphalt tile. Aluminum nosing at the edges of the tiers accents the vertical dimension. Figure 5 shows the larger of the lecture rooms looking toward the rear.

A fixed lecture table in each room provides ample space even for very large pieces of demonstration equipment, as well as outlets for water, electricity, gas, and compressed air. Mounted conveniently on a single panel on the instructor's side of the lecture table are electrical outlets and all controls for the room lighting. Cables from this panel run to outlets on the side walls of the room, to others mounted in the ceiling over the lecture table, and to an outlet at the rear of the room. A variety of conductors connect this panel to the main distribution panel in the basement of the building. Switches mounted on the panel separately control the general room illumination (either bright or dim), the entrance illumination, and the illumination of lecture desk and blackboards. An interval timer mounted in the front wall of the lecture room above the lecture table is controlled by a switch at the end of a retractable extension cable connected to the same panel.

Movable tables in both lecture rooms make it possible to extend the area available on which to mount apparatus and to move whole exhibits conveniently to and from the preparation room. Lantern screens are fixed in both rooms, although a portable 16-mm movie projector and a variety of slide projectors are used interchangeably in the two lecture rooms. A common preparation room for lecture demonstrations adjoins both lecture rooms, from which apparatus may be withdrawn as needed. The preparation room is provided with glass-enclosed cases, cupboards, drawers, a slide file, sink, electrical distribution panel, and tables. Some of the experimental circuits in the preparation room are connected permanently in parallel with correspondingly numbered outlets in the lecture rooms. The building elevator opens into the preparation room, providing means for transferring heavy equipment from the basement shops to the lecture rooms.

Most of the elementary laboratories on the first floor measure 22 ft by 40 ft. A typical laboratory, as shown in Fig. 6, was designed for use by a group of 20 students at one time. Windows running the length of each laboratory along one side of the room from table height to the ceiling make each laboratory a bright and cheerful place to work. Here, and throughout the building, Venetian blinds are standard equipment. Black-topped tables 30 in. high are provided upon which the students perform most of the experiments. Five service islands, also 30 in. high, in the center

of each laboratory offer outlets for electricity, gas, and water. In a typical elementary laboratory the only *fixed* pieces of furniture are the five service islands referred to, and a sink and table along one wall. Beneath the windows a row of narrow tables, also 30 in. high, is used in place of the usual fixed shelf. A blackboard is included in each laboratory for use in the short discussion which usually precedes the performance of an experiment. An instructor's desk and chair are

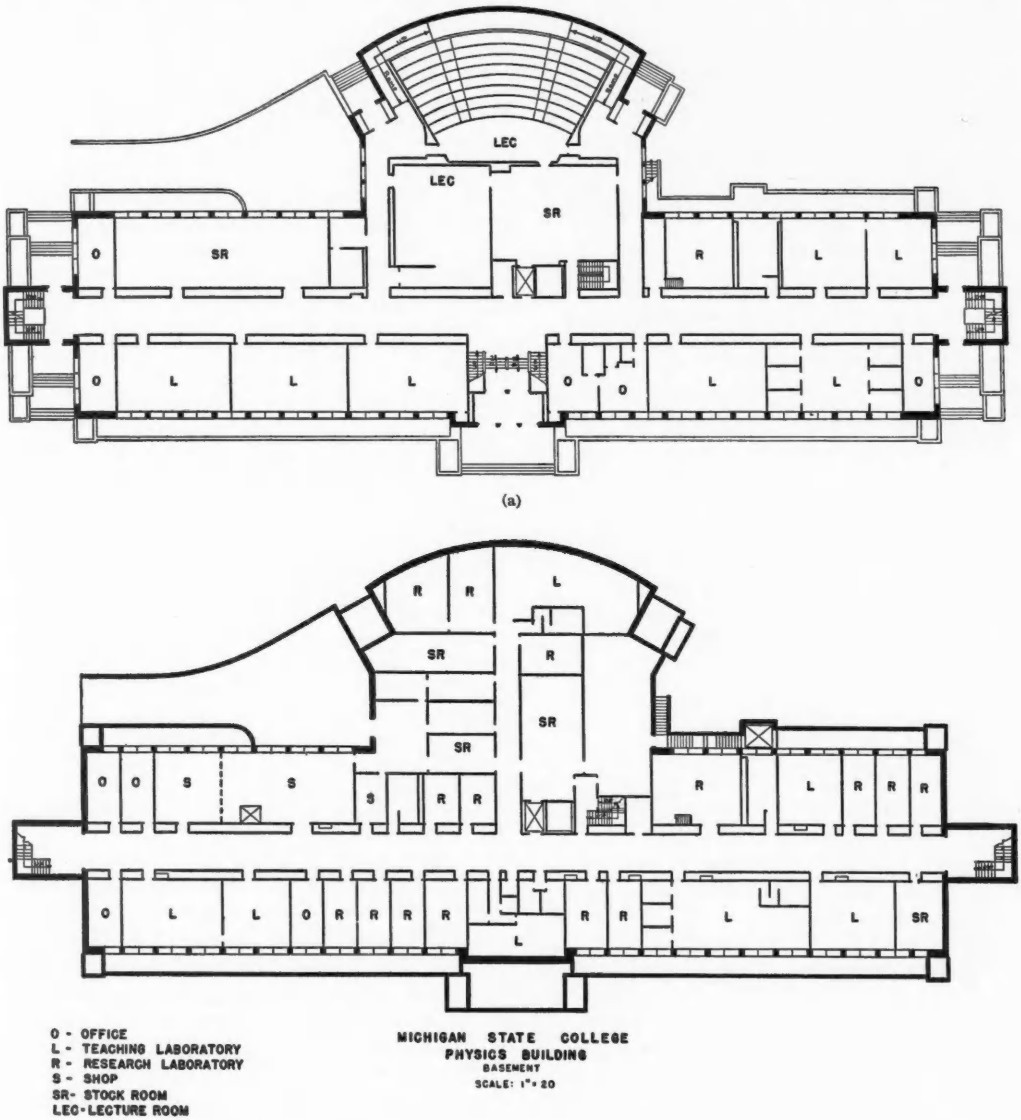

FIG. 4. Floor plans of the Physics-Mathematics Building. (a) First-floor plan. (b) Basement-floor plan. The rooms are indicated as follows: LEC—lecture room; L—teaching laboratory; R—research laboratory; O—office; S—shop; and SR—storage room.

FIG. 5. The large lecture room in the Physics-Mathematics Building. The lecture desk is visible in the right foreground; the barrier wall dividing the room from the entrance ramp is visible in the rear of the room.

standard equipment. For keeping current supplies and tools a cabinet having shelves and drawers is placed in each laboratory. Furniture in the laboratories is of birch; walls are painted ivory color; the floors are of marbleized asphalt tile. All laboratories have acoustically absorbent ceilings and fluorescent lighting.

The electrical subdistribution panels in the laboratories, from which the outlets on the service islands and walls are fed, are placed conveniently near to the instructor's desk. Separate switches and fuses give him considerable control over the currents which can be withdrawn from the electrical outlets by the students. Steel channels (Unistrut) in the walls and ceilings of the elementary laboratories may be utilized for mounting fixed apparatus at these points. Two compressed air cocks on the walls provide this service when needed. A coat rack for student use is built onto the wall of each laboratory.

One laboratory (22 ft by 48 ft) differs from the others in that it is subdivided into a central section and six smaller cubicles, each about 7 ft by 8 ft. The central section and the cubicles are all provided with light-tight shades making this laboratory very suitable for optics experiments requiring partially or completely darkened rooms. Another elementary laboratory (22 ft by 48 ft) is subdivided into two smaller rooms by a wall and an intercommunicating door. This laboratory, intended for use of the home economics students taking a special physics course, is provided with storage cases, since much of the apparatus used is specially designed for the course.

The storage room for the laboratory apparatus (22 ft by 71 ft) is intended to serve several purposes. In addition to the glass-enclosed cases which provide storage space for all of the apparatus which it is expected will be needed in the elementary laboratories, stocks of supplies, such as stoppers, beakers, paper, etc., are kept here in drawers and cases. A work table provides the space for the full-time technician to make minor repairs and adjustments to apparatus. A sink makes it possible to clean apparatus and mix chemicals. One novelty of the laboratory apparatus storage room is that it is an exterior room and has windows along a full side. These make it a pleasanter place to work than many apparatus storage rooms.

The office of the Department of Physics and Astronomy is adjacent to the first-floor foyer. In this suite are provided the space to carry on the business of the department, working space for the secretarial staff, a private office for the department chairman, a small mimeograph room, and a storage closet for supplies. The editorial activities for the *American Journal of Physics* are also carried on in this suite. The majority of the staff offices measure $10\frac{1}{2}$ ft by 22 ft. Each is provided with desks, bookshelves, a coat closet, a large table, files, blackboard, etc. Many of the offices have windows along a full side, providing a maximum of light. Walls of all offices are of cinder block finished in ivory shade. Several of the staff offices have floor tiles in an attractive shade of green. Fluorescent lighting and an acoustical ceiling are employed in each office.

The second floor of the building houses the office of the Department of Mathematics, seven staff offices, five classrooms, four laboratories, a library, a conference room, two computing rooms, and two photographic darkrooms. The office of the Mathematics Department is the mirror image of that of the Physics and Astronomy Department on the first floor. Staff offices on the second floor are also quite similar to those on the first floor. The laboratories for electrical measurements and advanced laboratory instruction on this floor are each provided with storage cases for keeping the special apparatus required. The

library is intended to provide space for 15,000 volumes on physics, mathematics, and astronomy. Fifty students may work here at a time. Books and periodicals are kept upon open shelves with a trained librarian available during the day and certain evening hours to help students search out needed information.

The conference room provides an informal setting for meetings and seminars. This room, measuring 39 ft by 27 ft, has sage green walls with a wainscoting of blond oak. Windows along one curved side look out upon the roof above the large first-floor lecture room. Furnishings in this room consist of plastic-covered circle sofas and chairs—some green, some yellow, and some flame-colored—with occasional tables of blond or of ebony finish. The normal seating capacity of the conference room is 25, although when necessary, folding chairs upholstered in a gay yellow plastic may be utilized to increase the capacity to 80. A concealed blackboard is built into one wall of the room. Draperies that carry out the strong modern color scheme may be drawn over the windows when it is desired to darken the room for use of a projector. A kitchenette and cloakroom adjoin the conference room.

In addition to the classrooms and offices on the third floor of the Physics-Mathematics Building, there is the astronomy laboratory and a proposed meteorology laboratory. The astronomy laboratory measures 22 ft by 37 ft and has glass-enclosed cases built into one entire side wall. A separate storage room and a darkroom adjoin this laboratory. A flat tiled roof covering more than half the area of the building will provide space for mounting the department's telescopes as well as some of the meteorological apparatus.

The basement of the Physics-Mathematics Building having a floor area of approximately 24,000 square feet serves the needs of the department for research, laboratory, shop, and storage space. A floor plan of the basement is shown in Fig. 4b. Sixteen research laboratories varying in size from 11 ft by 22 ft to 22 ft by 28 ft are available for use in the various research programs. One of these laboratories having a ceiling height of 25 ft is to be used for research requiring large-scale apparatus. A number of the other laboratories have special features built into them, e.g., a chemical ventilating hood or an acoustically treated interior. Although most of the research laboratories have ample window area to make them light and cheerful places in which to work, five laboratories have purposely been left without windows for use where experimental conditions require darkness.

FIG. 6. Typical elementary laboratory in the Physics-Mathematics Building. The service islands between the tables provide outlets for water, gas, and electricity.

Instructional laboratories in the basement of the building include one for spectroscopy, one for optics, two for advanced mechanics, heat, and acoustics, one for x-rays, and one for photography. Storage cases are generally provided in each of the advanced laboratories, making them more or less independent of the storage rooms on the first floor. These laboratories differ considerably from each other and from the elementary laboratories on the first floor depending upon the use for which they are intended. The advanced optics laboratory, for instance, is provided with three cubicles which may be darkened and with a photographic darkroom. The special photographic laboratory is arranged with stalls for twelve students to work at a time. Separate enlarging and developing rooms are included in the photographic-instruction unit.

For constructing the special equipment needed in the department's research and instructional activities in instrument shop 22 ft by 44 ft is included in the basement. Separated from the main shop by a screen is a student shop in which are to be found machines duplicating most of those in the main shop. A small wood-working shop is available, separate from the machine

shop. A truck ramp adjacent to the shops serves to facilitate delivery of heavy equipment to the building. Almost 1900 square feet of space in four rooms serve the needs of the department for storage space for stock, equipment, and parts.

Occupying three basement rooms is an electrical distribution system, which was designed and installed by the Standard Electric Time Company of Springfield, Massachusetts. One room contains the main electrical distribution board by means of which can be produced a wide variety of service combinations for various current and voltage requirements. This board receives from the college power plant 230-volt, 3 phase, and 230-volt, split phase (center-grounded) power. Controls on this board operate a pair of d.c. generators in a separate room. These generators can each furnish 150-volt, 75-ampere service. An adjacent room contains a battery of 120 lead storage cells, which are also connected into the main distribution panel. Half of the storage cells are connected in series for a 120-volt d.c. source. Leads from other groups of cells are brought into the board so that they, too, can be connected in series, or a number of groups of similar voltage may be connected in parallel for high current demand. The generators may, of course, be used to recharge the storage cells. Two Tungar rectifiers—one for 6 amperes and the other for 15 amperes—are also available for recharging duty. A 1000-cycle a.c. source rated at 10 volts, 4 amperes is available from the main distribution panel.

Each of these electrical services may be made available to the research and instructional laboratories by means of the distribution circuits running from the main distribution panel to the laboratory panels. Fifty-two of these circuits are trunk lines leading to eleven subdistribution panels in the instructional laboratories. Twenty-four more are heavy-duty (100-ampere) lines leading to research rooms on the basement level. Forty-two are coaxial lines for interconnecting research or teaching laboratories for high frequency work. Two coaxial lines terminate near the roof for use with antennas. The remainder of the standard (15 ampere) branch lines lead to the lecture or recitation rooms, apparatus stock rooms, or research laboratories. There are one hundred and ten standard branch circuits. Service outlets provide 115-volt a.c. at intervals along the walls of all laboratories, classrooms, and offices, so that no location along these walls is more than six ft from an outlet.

Mounting of apparatus in the laboratories and lecture rooms has been materially simplified by the insertion of steel channels (Unistrut) into the walls and ceilings. While it is obvious that no amount of planning can ensure placement of the channels in all positions where they might be desired, liberal use of them has made it possible to attach apparatus to most laboratory walls and ceilings with ease. The channels are set flush with the walls and ceilings and are inconspicuous. Spring clips fit into these channels quite simply making it possible to attach any piece of apparatus or any panel capable of being held by $\frac{1}{4}$-in. bolts. Channels in the laboratories are generally arranged horizontally at heights of 45 in. and 75 in. from the floor. Those between the windows run vertically from just the window ledge to a height of about $7\frac{1}{2}$ ft. The ceiling channels usually run the length of the room. Two channels over the lecture-room desks and one on each of the walls behind the lecture-room desks are also provided.

Although the Physics-Mathematics Building has more than 320 windows, it is completely independent of these for ventilation. Fresh air is drawn into the building from intakes located on the roof. A shaft conducts this air to the subbasement where it is washed and filtered. After the air has been heated in winter, or cooled in summer, it is forced into corridors, classrooms, laboratories, and workrooms through ducts in the walls of the building. Wherever possible the fresh air is fed into the rooms through full-length grilles in the quarry-tile window sills. Stale air is exhausted from the rooms through wall louvres located near floor level. All heating controls are automatic.

Numerous details have been incorporated into the furnishing of the building which make it an agreeable and convenient place in which to teach or study. Fluorescent lighting has been used exclusively in classrooms, laboratories, offices, and shops. Acoustical ceilings have been employed in rooms wherever people congregate or work. Floors of asphalt tile have been used except in stockrooms and shops. A coat rack for student use is to

be found in every classroom and instructional laboratory. Blackboards have been built into every classroom, laboratory, and office. Clocks controlled from a master clock are to be found in every corridor, classroom, and laboratory.

The careful planning on the part of the recent department chairman, Dr. Thomas H. Osgood, and others in the department who set the standards which the building was to achieve, together with the close cooperation between the architect and the department have begun to earn rewards in the form of the attractiveness and usefulness of the structure. Despite occasionally voiced regrets that some feature or detail desired by the speaker is missing, the Physics-Mathematics Building appears capable of fulfilling the basic requirements of the two departments for a long period to come, perhaps even to the venerable age at which the older building was retired from its duties. Meanwhile, visitors to and regular users of the building are outspoken in their admiration of its appearance, convenience, and utility.

Development of an Aspheric Lens Surface

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NONSPHERICAL lens surfaces are becoming increasingly important in producing better corrected optical instruments. One problem connected with the design of a nonspherical lens is that of developing a surface contour such that all rays emanating from a point source on the lens axis will emerge parallel to the axis after passing through the lens. The geometrical derivation of an aspheric lens surface is described in this paper. A mechanical linkage is proposed which traces the contour of the surface (a hyperbola) based upon this geometrical development.

The problem is to give the first lens surface, Fig. 1, a contour such that all rays emanating from source F will emerge parallel to axis FN. The second lens surface is plane. The condition that PS is parallel to FN is assumed in thin-lens theory when F is located at a distance from the lens equal to the focal length. If the slope of tangent TT' to the lens surface can be found for any given ray FP, such that PS will be parallel to FN when the ray emerges, the lens surface may be determined from a series of such tangents for a respective series of rays emanating from F by fitting them together into a curve. It is thus necessary to determine values of angle α corresponding to a series of values for angle δ.

Let NN' be the normal to the lens surface and be perpendicular to TT'. The slope of tangent TT' for a given value of δ is derived as follows:

Let i be the angle of incidence, r the angle of refraction, n the index of refraction. Then $i=\delta+r$, and

$$n=\sin i/\sin r=\sin(\delta+r)/\sin r, \tag{1}$$

$$n\sin r=\sin(\delta+r)=\sin\delta\cos r+\cos\delta\sin r. \tag{2}$$

Dividing through by $\cos r$ gives

$$n\tan r=\sin\delta+\cos\delta\tan r, \tag{3}$$

whence

$$\tan r=\sin\delta/(n-\cos\delta). \tag{4}$$

Since r and α are complementary angles,

$$\cot\alpha=\tan r=\sin\delta/(n-\cos\delta). \tag{5}$$

In Fig. 2, lay off FO as the focal length of the lens whose surface is to be determined. Erect QO perpendicular to FO at O. Line QO is tangent to the lens surface at O. Draw rays at δ, 2δ, 3δ, etc., at equal angular intervals emanating from source F. A convenient value for δ is 10 degrees. Using Eq. (5), determine the slope α, of tangent TT' for the selected values of δ, 2δ, 3δ, etc., for the particular index of refraction n, of the lens material to be used. Table I gives, as an example, values for an index of 1.50. Angles are calculated to the nearest minute of arc.

In Fig. 2, the slopes of rays emanating from F, and their corresponding slopes of TT' (Figs. 1 and 2) are now known. The points of intersection P_1, P_2, P_3, etc., of these rays and their respective slopes are found as follows: Let AA' (Fig. 2)

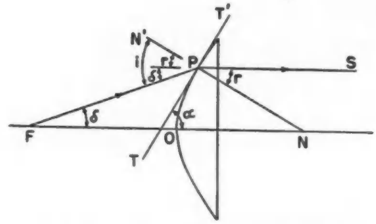

FIG. 1. Development of aspheric surface.

bisect angle δ, BB' bisect angle $2\delta-\delta$, CC' bisect angle $3\delta-2\delta$, etc. From Table I, the slope of tangent TT' (Fig. 1) for $\delta=10°$, is 71°23′. This tangent is drawn through S_1 (Fig. 2), the intersection of AA' and QO. Then P_1, the intersection of this tangent with the δ-ray from F, is a point on the lens surface.

For the next point, the slope of the tangent for $2\delta=20°$, is 58°37′ (from Table I). This tangent is drawn through S_2, the intersection of the first tangent with BB'. Another point on the lens curve is then P_2, the intersection of this second tangent with the 2δ-ray emanating from F. This procedure is continued until sufficient points have been found to draw the curve.

It can be shown analytically that this curve is a Cartesian hyperbola. Since this procedure is rather long an analysis by geometrical construction is shown in Fig. 3. Taking point P_3 as an example, construct angle ϕ_2 equal to angle ϕ_1, which is the angle between ray FP_3 and the tangent to the curve at point P_3. When this procedure is followed for each of the various points on the curve, the constructed arms of these angles converge at a point F' on the axis FO, extended. Point F' is the focus of the hyperbolic curve. The other arm of the hyperbola is sketched in for reference.

The mechanical linkage shown schematically in Fig. 4 is proposed for drawing the hyperbola for

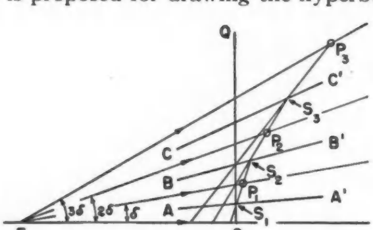

FIG. 2. Geometrical construction of curve.

any particular lens surface or for being built onto a standard lathe to cut such a lens surface from plastic.[1] Although the linkage has not been built, its description here will help clarify the relationships involved in the preceding geometrical development of the lens surface from which the construction of the linkage follows. Various other linkages that have been developed for drawing a hyperbola are not as well adapted for setting in the lens constants, particularly the index of refraction.

Link 1 represents the central ray through the lens. It is also the lathe axis of rotation when the lens is cut. The point O is on the lens surface and on the axis, and is the starting point for the cutting process. A length OF is set off as the focal length of the lens. The index of refraction of the lens material is set off in links 7A and 7B with the length of link 6A representing unity. These relationships are derived from Smith's Ray Plotter.[2] The tool post on the crossfeed of the lathe is at K. As K moves from O toward Q, the cutting tool at P, starting from O, cuts a Cartesian hyperbola as the lens surface. Point F is the focal point from which rays, such as link 3, emanate. All rays passing through the lens, such as link 8 extended, are parallel to the axis FO.

The linkage consists of four parts:

Part 1.—The basic structure of the linkage consists of FO, the center line of the lens and the lathe, OQ representing the direction of travel of the crossfeed on the lathe compound, and link 6B which maintains tangency to the curve at point P as K slides along OQ.

Part 2.—Links 1, 2, 3, 4, and 5 account for bisecting the angle PFO (compare procedure in

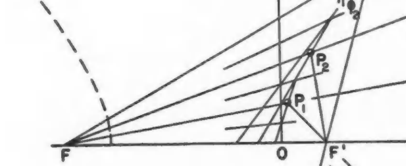

FIG. 3. Proof that curve is hyperbola.

[1] Since its development, this procedure has been superseded for quantity production by plastic molding methods.
[2] *Trans. Opt. Society*, 21, No. 3 (1919–20).

geometrical development of curve). Connector L slides on link 2 which always bisects angle PFO regardless of the position of link 3 pivoting about point F. Conversely, with link 1 rigid, K sliding along OQ positions link 2 which in turn positions link 3 upon which P slides. In practice, the driving force along OQ would be applied to link 3 instead of link 2.

Part 3.—Links $6A$, $7A$, $7B$, and 8 contribute the part that Table I played in the point-by-point method. Links $6A$ and $6B$ are rigidly connected and are at right angles with each other. Link $6B$, sliding through K, represents the tangent to the curve at point P, while link $6A$ is the corresponding normal. Point P slides on link 3 as K moves along OQ. Link 8 swivels at P and slides through N as K moves on OQ.

Part 4.—A requirement for the proper functioning of the optical ray tracing linkage discussed in Part 3, is that link 8 must always be parallel to FO. This is taken care of by the parallel linkage at the right in Fig. 4, which is constructed like a drafting machine.

The complexity of the linkage system necessitates a study of vector forces so that driving power can be applied to the system at various points during the hyperbola development to prevent links from binding or approaching locking position.

The suggestions and criticisms of Dr. G. M. Rassweiler of General Motors Research Laboratory throughout the work on this project are gratefully acknowledged and appreciated.

TABLE I. Representative values of α for $n=1.50$.

δ	$\sin\delta$	$\cos\delta$	$\cot\alpha$	α
0°	0.000000	1.000000	0.0000	90°00′
10°	0.173648	0.984808	0.3367	71°23′
20°	0.342020	0.939693	0.6100	58°37′
30°	0.500000	0.866025	0.7895	51°42′
40°	0.642788	0.766044	0.8754	48°48′

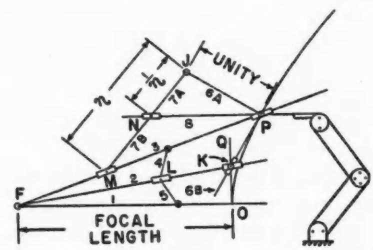

FIG. 4. Mechanical linkage to draw curve.

Unfulfilled Aspiration

The aims of the physicist, however, are in part purely intellectual: he strives to understand the Universe on account of the intellectual pleasure derived from the pursuit, but he is upheld in it by the knowledge that the study of nature's secrets is the ordained method by which the greatest good and happiness shall finally come to the human race.

Where, then, are the great laboratories of research in this city, in this country, nay, in the world? We see a few miserable structures here and there occupied by a few starving professors who are nobly striving to do the best with the feeble means at their disposal. But where in the world is the institute of pure research in any department of science with an income of $100,000,000 per year? Where can the discoverer in pure science earn more than the wages of a day laborer or cook? But $100,000,000 per year is but the price of an army or of a navy designed to kill other people. Just think of it, that one per cent of this sum seems to most people too great to save our children and descendants from misery and even death!

But the twentieth century is near—may we not hope for better things before its end? May we not hope to influence the public in this direction?—HENRY A. ROWLAND.

The Induction Kilowatt-Hour Meter

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IN our general physics course for nonscience students, we have for some years given the students a chance to "experiment" with electrical equipment usually found in the home. We have arranged a panel (Fig. 1) to simulate parts of a house with the main entrance switch and fuse block, kilowatt-hour meter, and two branch fuse blocks; one circuit leading to an ordinary double flush receptacle and SPST snap switch, the other circuit consisting of a lamp to be controlled by two three-way switches. A push button, transformer, and door bell, or gong, are also included. Regular commercial fixtures, switches, fuses, etc., are used wherever possible. In addition, an auxiliary fuse block is included so that the students may replace their own fuse wire when necessary. The fuse block has a mica cover for safety reasons, and saves blowing the regular screw fuses. The students are first instructed on commercial practices, safety precautions, tight connections, soldered joints, wire sizes, insulation, conduit, etc. Pliers, screw drivers, No. 18 stranded wire, tape, etc., are then furnished and the instructions are to "go to it." The wiring is not supervised by the laboratory instructor and so far as time permits the trial-and-error method is used. An interesting comment often turns up in the reports,—"Our experiments are usually a misnomer, this is the first one where we really experimented"!

The only quantitative part of the experiment is a determination of the constant of the kilowatt-hour meter. It is my feeling that this is a much neglected member of the family of electrical measuring instruments. We have students study the Wheatstone bridge and the reflecting galvanometer (not to mention the tangent galvanometer as a current-measuring instrument!) and yet the only electrical measuring instrument the majority of the students will ever have access to is the kilowatt-hour meter at home.

Assuming a constant line voltage of 110–115 volts, the students can make a reasonably accurate determination of the constant of the meter by loading the circuit with lamps of known wattage. However, in our laboratory the voltage regulation is so poor that we usually have the students measure the wattage by the voltmeter-ammeter method. Assuming a pure resistive load, the power factor may be taken as unity.

Data taken from a student report are plotted in Fig. 2. An analysis of the data is most inter-

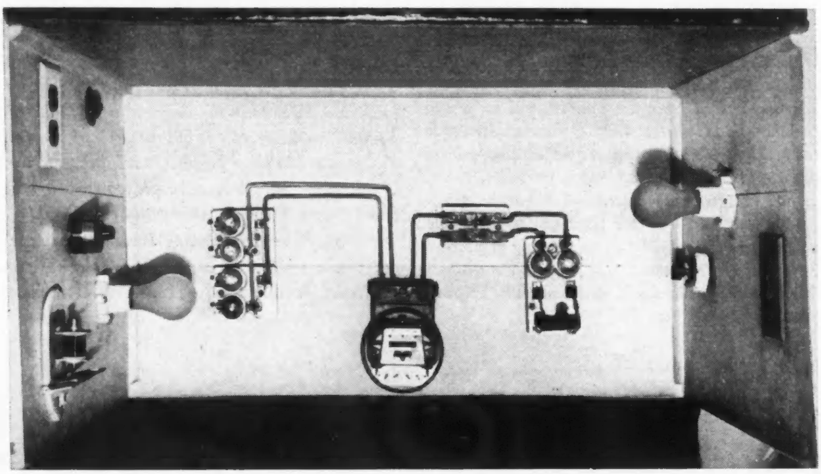

FIG. 1. The "house" ready for wiring.

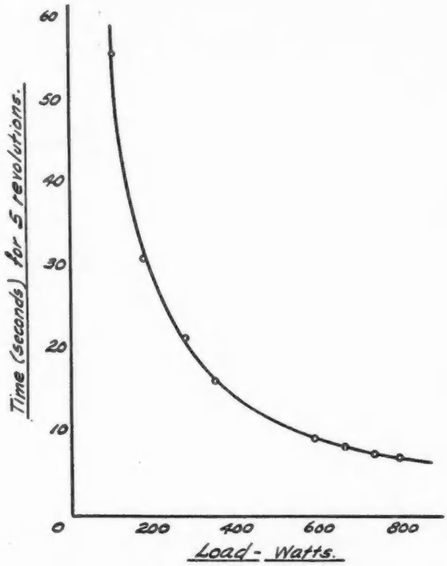

FIG. 2. Graph of load *vs.* time for disk to make five revolutions.

esting and instructive. The graph is strangely like that found in studying Boyle's Law, and it can be recalled that the area below the curve on the $P-V$ diagram represents energy. Similarly, area in Fig. 2 represents energy since it is the product of power and time. The energy per revolution of the disk is the constant of the meter, and is easily calculated from the data, yielding the results in Table I.

The constant for most of the new-type meters is stamped on the face of the instrument, or printed inside the cover over the screw connections. For the Westinghouse, Type OB single phase 5-ampere 60-cycle meters which we use, the constant is stamped $K_h = \frac{1}{3}$ watt hour/rev. This, of course, means that the disk must turn 3000 times to step the first dial up one kilowatt hour. Knowing the constant of the meter, it is a convenient way in a home to determine the load merely by timing the disk for a few turns.

We have several meters stripped of everything except the two field coils and the disk so that the operation of the motor may be studied. Just why the disk turns at all, since it has neither brushes nor slip rings, is a mystery to most beginners in college physics. A careful analysis of the meter is not difficult even for students in elementary physics.

Referring to Fig. 3a, the two coils A and B carry the line current. The disk is shown in cross section. The complete magnetic circuit is not shown, but shows up nicely in Fig. 4. The

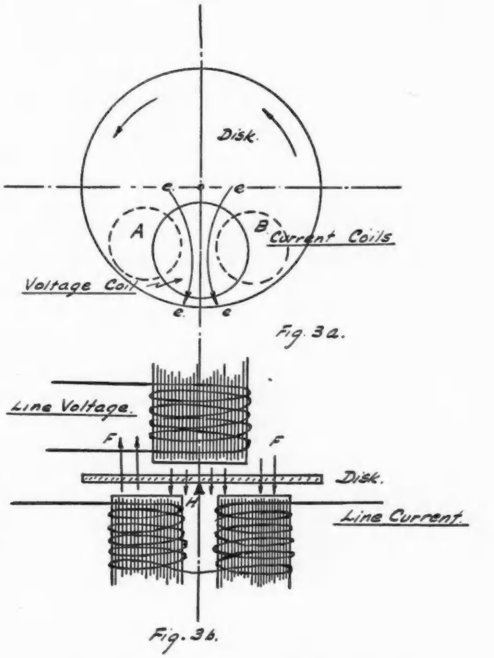

FIG. 3. Kilowatt-hour meter circuit.

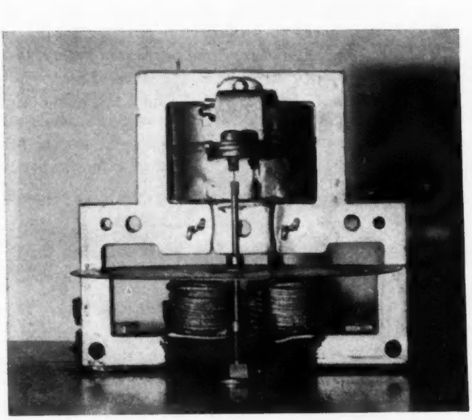

FIG. 4. Kilowatt-hour meter, stripped of gears, and damping magnet, showing complete magnetic circuit.

TABLE I. Constant of kilowatt-hour meter calculated from a student's data.

Load (watts)	Speed (rev/sec)	Constant (watt-hr/rev)
104.5	0.089	0.326
174.0	0.160	0.304
276.0	0.230	0.330
342.0	0.307	0.310
582.0	0.516	0.314
660.0	0.578	0.318
732.0	0.647	0.316
791.0	0.704	0.312
		Av. 0.316

changing magnetic field from coils A and B cuts through the disk as shown by arrows FF producing eddy currents *ee*. These eddy currents are 90° behind the line current since voltage is proportional to $d\phi/dt$.

The eddy currents themselves are not sufficient to turn the disk, but must flow through a suitable field. The field is supplied by the voltage coil placed centrally over the disk as shown. This coil is connected across the line and its field is a measure of line voltage. Since it is highly inductive (being always across the line) the current in it is 90° behind the emf. Hence the eddy currents *ee* and the field H are in phase with each other (assuming a resistive load) and the torque on the disk is proportional to EI. If the power factor is not unity, the torque will be $EI\cos\theta$, since only the power component of current will be in phase with the field produced by the voltage. The voltage coil H also produces eddy currents, but the field produced by this coil is small because of the small current in the highly inductive circuit; so these are neglected in comparison with the field set up by the current coils AB.

The direction of rotation of the disk is now easily gotten from Fleming's left-hand rule (conventional currents). The eddy currents at the instant shown are toward the reader in Fig. 3b and the field is downward, so the torque is counter clockwise, as shown in Fig. 3a.

The opposing torque is supplied by a permanent magnet so that the disk moves at constant speed under load, and also comes to rest without coasting when the load is turned off. Meters are usually supplied with a small "shading" disk to compensate for friction, but the newest types make use of a completely floating disk.

Besides ministering to our comfort, science also serves to satisfy certain needs of the human spirit. It helps us to understand the world and to feel at home in it. We are distressed by disorder, and always try to arrange in order the things with which we have to deal, whether they are the affairs of a nation, the books in a library, or our own ideas. Science satisfies us because it shows us that, behind the transient and confused pageant of nature, there is a permanent and orderly reality. And it was in the sky that this order was first revealed to men on a vast and spectacular scale.—A. ARMITAGE, *Sun Stand Thou Still* (H. Schuman, New York, 1946).

RECENT MEETINGS

Oregon Section

The fifty-third meeting of the Oregon Section of the American Association of Physics Teachers was held at Willamette University, Salem, Oregon, on February 18, 1950. DR. WILLIAM R. VARNER, President of the Section, presided at the meeting. MR. F. W. DECKER described the U. S. Air Force Reserve training program in meteorology with special reference to students interested in specialist training as observers and forecasters with the Air Weather Service.

The President welcomed PROFESSOR MARTIN E. NELSON and PROFESSOR RAYMOND S. SEWARD, both of the *College of Puget Sound*, to the Section. In addition, the following members of the Oregon Section registered for the fifty-third meeting: *Albany High School*, J. E. Lunn; *Corvallis High School*, P. N. Spelbrink; *Lewis and Clark College*, A. A. Groening, B. R. Huffstutter, J. B. Krauss, D. D. Loomis; *Linfield College*, George Barnes, Walter Dyke; *Lowell Union High School*, Clarence L. Miller; *Oregon State College*, Duis Bolinger, J. J. Brady, Cleo C. Byers, C. L. Church, John A. Day, Fred W. Decker, J. W. Griffith, D. M. Holm, Molly Holm, Russell Lincoln, R. D. Merritt, F. B. Morgan, William R. Varner, H. R. Vinyard, W. Weniger; *Reed College*, K. E. Davis, Laurence Germain, W. L. Parker, A. A. Knowlton, Leo Seren; *Salem Senior High School*, June Philpott; *University of Oregon*, A. E. Caswell, F. E. Dart, R. T. Ellickson, Will V. Norris; *University of Portland*, Rev. Richard D. Murphy, M. A. Starr, Paul Wack; *Vanport Extension Center*, Curtis E. Borchers, T. A. Shotwell, R. B. Walton; *Weyerhaueser Timber Company*, E. Hobart Collins; *Willamette University*, Rob Beal, R. B. Bennett, E. T. Brown, Lawrence Cherry, F. G. Clemans, Ernest Diebrich, R. L. Graham, G. E. Halliday, C. F. Luther, G. A. Odgers, Jr., Phil Phipps, R. L. Purbrick, John Thompson, Allen Wilcox, Ralph Wilson.

A program of invited and contributed papers was presented as follows:

Reflection of microwaves from metal plate structures. JAMES J. BRADY, *Oregon State College*.—Electromagnetic waves confined between conducting plates which are parallel to the electric vector and spaced more than a half-wavelength apart have a phase velocity greater than their free-space velocity. A row of such parallel plates constitutes a refractive medium with an index of refraction $n=[1-(\lambda/2a)^2]^{\frac{1}{2}}$, where a is the plate spacing. A similar structure has been constructed by stacking equal lengths of wave guides of square cross section. Wooden wedges were inserted into the exit ends of the guides to lower the reflections from back surfaces. Experiments indicated that this was very effective. Data for the reflection coefficient were obtained as a function of the angle of incidence for indices of refraction ranging from $n=0.51$ to $n=0.67$. Starting from a small angle of incidence ($\theta=10°$) the reflection coefficient decreased to $\theta=\text{arc sin}(\lambda a^{-1}-1)$. The coefficient then increased for θ greater than this value. It was found, also, that a secondary reflected lobe appeared at this particular value of θ, and its magnitude increased with increasing θ to a maximum and then decreased for further increase in the angle of incidence. Of special interest is the case of $\cos\theta=n$, in which half of the reflected energy is in the normal reflected lobe ($\theta=r$) and half in the secondary lobe. Without the wedges in the exit ends of the wave guide, the transmitted energy is likewise split into two lobes of equal energy for $\cos\theta=n$. A theory has been developed by Lengyel of the Naval Research Laboratory which accounts satisfactorily for the experimental results.

Angular dependence of inelastically scattered protons from Be^9. KENNETH E. DAVIS, *Reed College*.—As a result of the same work on the determination of nuclear energy levels through inelastic scattering of protons done at the University of Rochester, there was an indication of considerable variation of intensity with incident energy and with scattering angle. Further investigation seemed indicated and the newly discovered level in Be^9 seemed to be a promising subject.[1] Its sharpness and clear separation from the next-nearest level were important considerations in its favor. Using a modified form of a scattering camera once devised by Wilkins,[2] studies were made. The equipment used and some preliminary results were discussed.

[1] K. E. Davis and E. M. Hafner, *Physical Rev.* **73**, 1473 (1948).
[2] T. R. Wilkins and G. Kuerti, *Physical Rev.* **55**, 1134 (1939); T. R. Wilkins, *J. Appl. Physics* **11**, 44 (1940).

A Geiger counter for weak radiations. ROBERT B. BENNETT, *Willamette University*.—A Geiger counter system employing a scale-of-64 instrument was demonstrated with a Victoreen Thyrode. This instrument is to be used with a thin-window Geiger tube whose counting volume is variable. Slides showing the construction of the tube were presented and a tube was displayed. Possible advantages and characteristics were mentioned. The tube will first be used to detect C^{14} used as a biological tracer.

Scattering of elementary particles by nuclei. RAYMOND T. ELLICKSON, *University of Oregon*.—A brief description was given of the way in which data from nuclear scattering experiments are used to obtain information about the nature of nuclear forces.

Report on the nineteenth annual meeting of the AAPT. WILLIAM L. PARKER, *Reed College*.

The United States and Russia. ROBERT D. GREGG, *Willamette University*.

The mercury-indium molecule. ROBERT L. PURBRICK, *Willamette University*.—When mercury and indium are heated together in an evacuated quartz tube and the resulting gas is electrically excited, it is found that the spectrum of the light emitted contains systems of bands at wavelengths 4102A, 4511A, 4994A, 5226A, 5544A, and 5760A. These band systems were analyzed with the University of Wisconsin 21-ft reflection grating spectro-

graph. Formulas for the wave numbers of the band heads have been obtained except for the 5760A system. A description of the bands and their production was given.

Can religion be brought into the physics classroom? LEO SEREN, *Reed College.*—If we agree that there is but one God of the world and he is the Creator of all things and all knowledge, then all natural phenomena falling within any one group ought to be explainable by a single law appropriate to that group of phenomena. This seems to be the case for classical mechanics, electrostatics, electrodynamics, and thermodynamics. The appropriate laws are Newton's universal law of gravitation, Coulomb's law, Ampère's law, and Clausius' law. All of the subject matter now taught in these various fields can be taught by starting with the governing law and then developing the concepts. If we further agree with the metaphysical principle that when man observes nature he is observing himself as much as nature, then it follows that man obeys the law of the Creator. Bringing religion into the physics classroom thus provides the intellectual basis for good habits.

Directional broadcasting antennas. DWIGHT LOOMIS, *Lewis and Clark College.*—An essentially nontechnical discussion covering some of the reasons why the directional systems are in use was presented. Consideration was given to the classification of stations with respect to their service rendered and to the interference allowable. Two classes of service were discussed: namely, the new service and the extension of older services. A practical example was given for this latter case in the extension of a daytime service.

Arrangements were made for a meeting at the University of Portland, Portland, Oregon, on May 13, 1950.

FRED W. DECKER, *Secretary*

Kentucky Section

The annual spring meeting of the Kentucky Section of the American Association of Physics Teachers was held on April 14, 1950, in the Physics Building of the University of Louisville. The meeting, held in conjunction with the annual meeting of the Kentucky Education Association, was attended by 55 members and guests. Twenty-five of these were a delegation from Hanover College, Hanover, Indiana. DR. RALPH A. LORING, *University of Louisville,* presided.

The program consisted of an invited paper by DR. DUANE ROLLER, *Wabash College,* President of the American Association of Physics Teachers, on the subject, "Should physics really play a fundamental role in a liberal education?" Dr. Roller first briefly reviewed the growth and changes in our ideas about the relations of physics to the social fields and humanities that have occurred in the past several decades, and then described early as well as current attempts to provide physics instruction for intelligent laymen. An analysis was then given of the interconnections of physics and the other main fields of secular knowledge on three levels: the technologic, the conceptual, and the hierarchial, this last being concerned with the place of physics and its methodology in the whole hierarchy of secular knowledge. He contended that these interconnections provide the framework for really significant instruction of those students who are likely to be the future leaders in the social fields and humanities; and that any physics department, by developing such a course, can make an educational contribution that is likely to be regarded as valuable and outstanding by the students and faculty of the college as a whole. The course preferably should be given on the junior-senior level.

After the paper about 30 members had luncheon in one of the college cafeterias. Following the luncheon a business meeting was held at which the following officers were elected for 1950: *President,* WALDEMAR NOLL, *Berea College; Vice-President,* EARLAND RITCHIE, *Centre College; Representative to Executive Committee,* LOUIS A. PARDUE, *University of Kentucky; Secretary-Treasurer,* RICHARD HANAU, *University of Kentucky.*

RICHARD HANAU, *Secretary*

Michigan Teachers of College Physics

The spring meeting of the Michigan Teachers of College Physics was held on Saturday, May 13, 1950, at Michigan State College, East Lansing, Michigan in the new Physics-Mathematics Building. Approximately one hundred teachers from colleges and universities throughout the state came for the sessions. DR. EVERETT R. PHELPS, *Wayne University,* presided over the morning session; DR. THOMAS H. OSGOOD, *Michigan State College,* introduced the speaker at the afternoon session and opened the building for inspection. The principal address of the afternoon session was given by DR. JAMES N. SNYDER, *University of Illinois,* who spoke on the subject "Foundations of meson theory and its changing status."

A program of contributed papers was presented as follows:

Elementary physics applied to medical problems. ROBERT H. ESLING, *Wayne University.*

The gyroscope in elementary physics. E. F. BARKER AND P. F. BARKER, *University of Michigan.*

Experiences at Oak Ridge. WALTER L. WEEKS, *Michigan State College.*

A vapor pressure-temperature apparatus. R. L. JUDKINS, *Wayne University.*

Design and construction of an air-cooled electromagnet. ARTHUR LUCK, *Michigan State College.*

Quick determination of a dry cell condition. K. W. SAUNDERS, *Central Michigan College.*

Anisotropy of ferromagnetic materials. RICHARD KROPSCHOT, *Michigan State College.*

Thermal electromotive force. W. W. SLEATOR, *University of Michigan.*

Luncheon for the group was provided in the Michigan State College Union. For the ladies a conducted tour of the campus and a musical program were provided. The meeting closed with a tea and social hour in the conference room of the new building.

Chesapeake Section

The annual meeting of the Chesapeake (formerly the District of Columbia and Environs) Section of the American Association of Physics Teachers was held on March 25, 1950, at the Naval Ordnance Laboratory, White Oak, Maryland. One hundred members and guests were registered. The morning session was devoted to a symposium on classical physics, with invited speakers. In the afternoon a tour of the laboratories was followed by contributed papers and the annual business meeting.

Symposium

Is Classical Physics Dead?

Acoustics. J. C. HUBBARD, *Catholic University of America.*
Magnetism. L. R. MAXWELL, *Naval Ordnance Laboratory.*
Thermodynamics. F. G. BRICKWEDDE, *National Bureau of Standards.*

Contributed Papers

1. **A lecture-room optical disk.** H. E. CARR, *Alabama Polytechnic Institute*; W. T. FENHAGEN AND J. R. SMITHSON, *U. S. Naval Academy.*
2. **Velocity of sound at ultrasonic frequencies by spark photography.** W. J. THALER, J. A. FITZPATRICK, AND LAURA CHENG, *Catholic University of America.*
3. **Atmospheric twinkle studies in motion pictures.** C. P. BUTLER, *Naval Research Laboratory.*
4. **High precision spectroscopy of gamma-rays.** B. B. WATSON, *Division of Higher Education, U. S. Office of Education.*
5. **An electron beam deflection experiment.** L. MARTON AND J. A. SIMPSON, *National Bureau of Standards.*
6. **A laboratory experiment on the determination of gamma for gases by self-sustained oscillations.** W. F. KOEHLER, *U. S. Naval Postgraduate School.*
7. **Use of field tank in teaching electronics.** T. B. BROWN, *George Washington University.*
8. **A projection timer.** R. A. GOODWIN AND W. T. FENHAGEN, *U. S. Naval Academy.*
9. **Inferences in some ancient books (the Bible) of present-day physics.** MAURICE T. BRACKBILL, *Eastern Mennonite College.*
10. **A lecture demonstration for the three types of magnetic substances.** R. E. TRUMBLE, JR., *U. S. Naval Academy.*
11. **The Navy radiac program and civil defense.** J. CRYDEN, *Bureau of Ships, Navy Department.*

At the business meeting it was voted to change the name from the "District of Columbia and Environs" to the "Chesapeake" Section of the American Association of Physics Teachers. The following officers were elected for the coming year: *President* and *Representative on the National Executive Committee*, E. R. PINKSTON, *U. S. Naval Academy; Secretary-Treasurer*, J. H. MCMILLEN, *Naval Ordnance Laboratory; Executive Committee:* VOLA P. BARTON, *Goucher College;* E. R. PINKSTON, *U. S. Naval Academy;* J. H. MCMILLEN, *Naval Ordnance Laboratory;* R. R. MEIJER, *George Washington University;* ALBERT MAY, *Naval Ordnance Laboratory*

E. R. PINKSTON, *President*

Chicago Section

The Chicago Section of the American Association of Physics Teachers held its spring meeting, May 13, 1950, at the Technological Institute of Northwestern University. PROFESSOR L. I. BOCKSTAHLER, *Northwestern University*, was the host, and DR. P. A. CONSTANTINIDES, *Wilson Branch, Chicago City Junior College*, was the chairman of the meeting. A program of invited papers was presented as follows:

Report of the New York meeting of the executive committee of AAPT. L. I. BOCKSTAHLER, *Northwestern University.*

Determination of *g* using electronic timing. HARALD C. JENSEN, *Lake Forest College.*

The curriculum in physics at the University of Chicago. HAROLD R. VOORHEES, *University of Chicago.*

Curriculum trends in the physical sciences at the University of Chicago. JAMES B. PARSONS, *University of Chicago.*

Luncheon was served in the cafeteria, Scott Hall. Another meeting is planned for the fall.

W. R. ANDERSON, *Secretary*

Southern California Section

The 1950 annual meeting of the Southern California Section of the American Association of Physics Teachers was held at Whittier College in Whittier, California, on Saturday, March 18, 1950. It was presided over by the President, DR. WILLARD GEER, *University of Southern California.* Sixty-six members and guests attended, practically all of whom stayed for the afternoon session.

The morning session began with a discussion of the High School Test Program of the Southern California Section led by three high school physics teachers: MR. J. B. FORSTER, *Mark Keppel High School*; MR. R. A. MCINTURFF, *El Monte Union High School*; and MR. G. R. TRACY, *Long Beach Polytechnic High School.* These teachers reported on the methods of preparation of students for the tests. Practically the only special training given was a rapid survey of the work not covered by the date of the test. Suggestions for the improvement of the test itself such as the inclusion of an essay type question involving reasoning were made.

The following ten-minute contributed papers were then presented:

A lens considered as a prism of variable angle. DAVID F. BENDER, *Whittier College.*—Many texts in elementary

physics introduce the subject of lenses by comparing half a positive lens with a prism, but fail to make further use of the idea. This concept can very easily be used to deduce the formula for the focal length of a thin lens by using the expression $D=(n-1)A$ for the deviation of a ray of light by a thin prism.

Oil well logging. HARLEY J. HADEN, *Glendale College.*—An oil well log is a graphical (or other) record of some physical phenomenon as a function of depth in a bore hole. Examples of types of logs include temperature, fluid velocity, electric potential, resistivity, gamma-ray, and neutron.

Education?—Or merely training?! VII. An analysis and an interpretation of corollary I of Newton's laws of motion. GEORGE FORSTER, *Pasadena City College.*—Concomitant with the practice of presenting opportunities to learn about Newton and other great intellects of science, we should also endeavor conscientiously to seek and to reveal the *truths* which those men sought to convey by their statements of principles, because education includes not only the acquiring of as broad knowledge as possible, but also the much more difficult task of developing profundity and the ability to penetrate, in order to gain mastery and insight. Did Newton really "enunciate" the so-called "principle of the parallelogram of forces," as claimed by Ernst Mach and his disciples? Or did they ascribe an idea to Newton which he may not even have had in mind, or to which he attached less importance than have many others?

Logic, history, and nuclear forces. F. W. WARBURTON, *University of Redlands.*

The general relation between phase and group velocities as illustrated by water waves. E. ALLAN WILLIAMS, *University of California at Santa Barbara.*—The relationship between phase and group velocities is illustrated for the cases: $U=V$, $U=V/2$, and $U=3V/2$; that is for group velocity equal to, less than, and greater than phase velocity by tide, deep water gravity waves, and ripples, respectively. These special cases are obtained from Rayleigh's general relationship $V=A\lambda^n$ by letting n take the values 0, $\frac{1}{2}$, and $-\frac{1}{2}$.

Floyd K. Richtmyer, 1881–1939, scientist, teacher, friend. F. R. HIRSH, JR., *Pasadena.*—The inherent ability of Richtmyer as a scientist was discussed, and his extraordinary teaching ability was commented on. The sincere friendship he manifested toward his associates was portrayed and a short poem to his memory presented.

Intensive study schedules. GILBERT MYERS, *Chadwick School.*

More paper for physics teachers. STANLEY C. PEARSON, *Muir College.*—An explanation and demonstration of the use of paper and cardboard in constructing visual aids which are large enough for classroom use were given. The aids include picture mounting, vernier scale, contour intervals, sine templet, and shot tube. In addition an octant which folds flat for easy storage and shows x, y, z components, direction angles, and direction cosines of vectors was shown.

A simple dynamical demonstration of $f=R/2$ for a concave spherical mirror. VERNON L. BOLLMAN, *Occidental College.*—A simple ball pendulum is hung directly over a horizontal concave spherical mirror and adjusted to contact the surface at all points when set into motion. A vertical beam of parallel light is reflected from the mirror indicating the principal focal point on the string which is then folded to show $f=R/2$.

A scoring device. MYRON S. ALLEN, *Long Beach City College.*—A novel device, now on the market, is demonstrated which, though small enough to carry in a brief case, will score 1000 multiple choice or true-false questions per minute. The scoring is done by perforating the answer forms for an entire class one question at a time. The rapid scoring permits the papers to be handed back and discussed immediately after the administration of the examination.

At the conclusion of the contributed papers, DR. DAVID L. SOLTAU, *University of Redlands,* gave a report upon the annual meetings of the American Association of Physics Teachers and its Executive Committee.

The afternoon program was an invited paper, "The physics of light nuclei," by PROFESSOR W. A. FOWLER, *California Institute of Technology.* This was a timely and capable presentation of the behavior of nuclei which results in the generation of large amounts of energy in stars and in the laboratory.

At the business meeting which followed new officers for the year 1950–51 were elected. These are: *President,* HARRY A. KIRKPATRICK, *Occidental College; Vice-President for colleges,* EARL C. REX, *Pepperdine College; Vice-President for junior colleges,* GEORGE WOOLSEY, *East Los Angeles Junior College; Vice-President for High Schools,* MRS. MARGARET Q. DAVIS, *Hamilton High School;* and *Secretary-Treasurer,* DAVID F. BENDER, *Whittier College.*

DAVID F. BENDER, *Secretary-Treasurer*

No great law in Natural Philosophy has ever been discovered for its practical applications, but the instances are innumerable of investigations apparently useless in the narrow sense of the word which have led to the most valuable results.—LORD KELVIN.

NOTES AND DISCUSSION

An Experiment on Malus' Law for the Elementary Laboratory

SANFORD C. GLADDEN
New Mexico College of Agriculture and Mechanic Arts, State College, New Mexico

EXPERIMENTS on polarization are usually lacking in the first college course in physics. Frequently the reason for the omission is that most such experiments do not provide quantitative results, unless apparatus beyond the means of the ordinary laboratory is available. One experiment that lends itself well to the facilities of the average laboratory is here described. The author claims no originality other than an adaptation of several techniques to the particular problem.

In 1809, the French physicist Etienne Louis Malus discovered that the intensity of the light transmitted by a polarizer and analyzer might be expressed in the form

$$I = I_{\max}\cos^2\theta, \qquad (1)$$

where I is the intensity of the transmitted light, $I_{\max}$ the maximum amount of light transmitted, and θ the angle of rotation of the analyzer from the position for which the transmitted light is a maximum. It is the study of this law with which the present experiment is concerned.

For polarizer and analyzer, pieces of Polaroid film are placed in metal lens mounts of about 2.5 cm inside diameter. A sheet of polar coordinate paper is mounted on a wood disk of about 20.0 cm diameter, to which is attached a 1.0-cm rod for supporting in an optical bench carriage. Two such mountings are required. A hole in the disk just large enough to permit easy turning of the lens mount serves to hold the polarizer or analyzer. A 60-w frosted lamp is employed as light source, and the indicating mechanism consists of a photoelectric cell in a shielded mount and an ordinary wall galvanometer. Both the lamp support and the photoelectric cell are attached to a 1.0-cm rod, and the entire system mounted on an optical bench. The particular cell used was an RCA Type 929, with a series resistance of 250,000 ohms and a 45-v B-battery.

Pointers are attached to the lens mounts for convenient reading of the position of polarizer and analyzer. In practice, the polarizer is set at some arbitrary position and the analyzer turned in 10° steps. The positions of the light source, polarizer, analyzer, and photoelectric cell are not critical, but will depend upon the characteristics of the particular photoelectric cell and galvanometer employed. For the data represented, the light source was 9.0 cm from the polarizer, the analyzer 14.0 cm from the polarizer, and the photoelectric cell 11.0 cm from the analyzer.

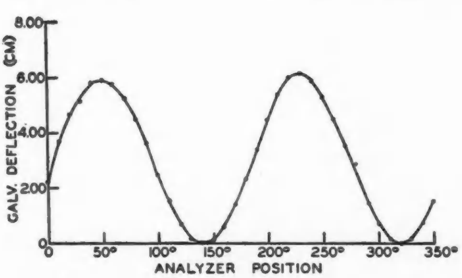

FIG. 1. Response of the photocell in the experiment on Malus' law.

Figure 1 represents the results of one set of observations. It will be noted that this curve locates the positions of maxima and minima, from which the data must be reinterpreted for the application of Malus' law. The information is replotted in Fig. 2 in order to show the application

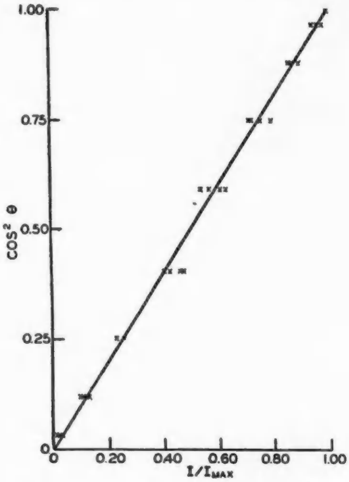

FIG. 2. Application of Malus' law to the results of the experiment.

of Malus' law. The two maxima agree closely, as do the two minima, and the separation between adjacent maxima or minima agrees with the theory.

It is believed that this experiment can be performed in most laboratories with apparatus conveniently available and relatively inexpensive. The student may therefore obtain a better appreciation of Malus' law, and in addition acquire desirable techniques.

A Simple Harmonic Motion Demonstrator

R. A. HINSHAW
Muskingum College, New Concord, Ohio

ONE of the more difficult concepts for the student in general college physics to visualize is the relationship between the rotary motion of a particle around a reference circle and the simple harmonic motion of the projection of this particle on a diameter of the circle.

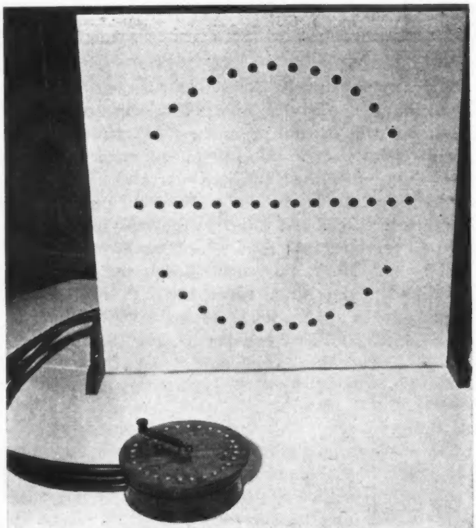

FIG. 1. Photograph of the apparatus demonstrating the relationship between rotary motion and simple harmonic motion.

Various authors of physics textbooks have suggested models such as the shadowgraph and the mechanical illustration to demonstrate the relationship.[1] The apparatus described below and built at Muskingum College, while probably not new in principle, does, we feel, portray clearly the desired relationship in an interesting manner.

The apparatus consists of a masonite board two-ft square containing 41 radio panel lights mounted in the manner shown in Fig. 1. The lights on the circle are red, while those on the diameter are yellow, with the exception of the two at the ends which are both on the diameter and the circumference and which are green. Of course, white lights could be used throughout with equal success. A three-ft cable connects the panel board to a rotary switch with 28 contacts. As the switch is rotated, the lights on the circle are lit in succession simultaneously with the light on the diameter indicating its projection.

[1] For example, E. Hausmann and E. P. Slack, *Physics*, third edition (D. Van Nostrand Company, New York, 1944), p. 160.

A Type of Examination in Physics

A. D. MISENER
University of Western Ontario, London, Canada

IN examining first-year students in general physics we have recently used a type of examination which has proved satisfactory to both teacher and student. These examinations are similar to those described by Rassweiler[1] but differ from them in several important features.

The examination consists of three parts.

Part A, total value 30 percent; questions on principles, laws, and definitions. The student answers all questions in this part.

Part B, total value 50 percent; questions involving one physical principle only and usually including a simple numerical problem.

Part C, total value 20 percent; questions involving a combination of physical principles with numerical problems. The student chooses one out of a group of three or four of these questions.

In Parts A and B the questions are so chosen that they form a representative selection from the whole subject matter of the course. The student is not asked to choose a small number of questions from a large assortment covering every topic dealt with. "Wide-choice" tests have two distinct defects; the student wastes valuable time making his selection, or the discerning student will cram up one-third of the course only, knowing that he will obtain good grades thereby.

Part C gives the good student his opportunity to distinguish himself from the average. The questions are not of great difficulty but are sufficiently complex to prevent the poor student from profiting by a lucky guess.

The most serious criticism of this type of test has been that the simple questions of Part A, which all but the poorest should answer, will influence the total mark to such an extent that it will no longer truly differentiate student ability. It was recognized that the possibility of a sure 30 percent is of psychological benefit to the student as he attempts Parts B and C but it was feared that the marks for Part A would cause the total mark for each student to come too close to the class average.

This is not the case. We have found, on analyzing the results of an examination given to a class of 147 first-year students, that there is an amazingly good correlation between the marks obtained for Part A and the total mark. The results are shown graphically in Fig. 1. The coefficient of correlation is defined as[2]

$$r=\frac{1}{\sigma_A\sigma_T}\left(\frac{\Sigma\Delta_A\Delta_T}{n}-\omega_A\omega_T\right)$$

FIG. 1. Graphical representation of the correlation between students' total examination scores and their scores for questions dealing only with principles, laws, and definitions.

with a dispersion of 0.67 $[(1-r^2)/(n)^{\frac{1}{2}}]$, where $\sigma_A =$ standard deviation of marks for Part A, $\sigma_T =$ standard deviation of total marks, $n =$ number of papers, $\Sigma\Delta_A\Delta_T =$ product moment, calculated from integral values of the average marks, ω_A and $\omega_T =$ the differences between the true averages and the integral values.

The results of this examination gave a coefficient of correlation $r=0.72$ and a dispersion of 0.027. As a general rule correlation is considered well marked if $r>0.5$ and $r>6$ times its dispersion. Therefore, the correlation found between the marks for Part A and the total marks may be described as excellent. This indicated that the students who do well on Part A also do well on the paper as a whole and that the students of inferior capacity are not unduly assisted by the presence of a section of the paper which is essentially memory work.

We concluded, therefore, that this type of examination gave a fair test of the students' knowledge of the course as a whole and also differentiated between good and poor students. It should be noted that the students in this class all had essentially the same preparation before starting the course.

[1] M. Rassweiler, *Am. J. Physics* 11, 223 (1943).
[2] L. Tuttle and J. Satterly, *The Theory of Measurements* (Longmans, 1925), Chapter XXI.

A Simple Electronic Spark Timer

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University of Buffalo, Buffalo, New York

A DEVICE which can produce a series of intense sparks occurring at equally spaced time intervals and capable of jumping gaps up to several millimeters in width is an essential piece of apparatus for accurate timing in both elementary and intermediate physics laboratories.[1] Basically such a spark timer consists of a spark coil, the primary of which is activated by current pulses fed to it at regularly spaced time intervals. Several types of spark timers are available commercially, all of which depend on some sort of mechanical interrupter to produce the current pulses in the primary of the spark coil. It is possible, however, to construct a spark timer which has no mechanical or moving parts. A number of such spark timers have been constructed by the authors. They are simple to build, rather inexpensive, have accurate time intervals, and have proved to be very satisfactory in the laboratory.

The spark timer described in this note is basically a conventional phase-shift-controlled thyratron rectifier[2] with the phase of the grid voltage set so that the thyratron fires very late in the cycle, thereby allowing current to

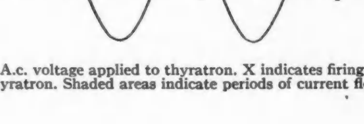

Fig. 1. A.c. voltage applied to thyratron. X indicates firing point of thyratron. Shaded areas indicate periods of current flow.

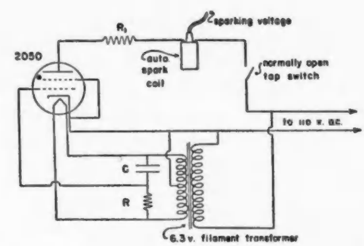

Fig. 2. Complete circuit diagram of electronic spark timer. The particular values of its elements are: Thyratron, Type 2050; transformer, 6.3 v, 1.2 amp center-tapped filament transformer; switch, single pole, single throw, normally open momentary or tap switch; spark coil, a standard automobile spark coil works very satisfactorily for this purpose; resistance R_1, 25 ohm, 50 watt dividohm; condenser C, 0.01 μf; resistance R, 600,000 ohm, $\frac{1}{2}$ watt.

flow in the circuit only for a very short time interval during each cycle, as indicated in Fig. 1. Thus the plate current of the thyratron consists of pulses of current of very short duration recurring every 1/60 sec (using 60-cycle power), the accuracy of the time interval being determined by the accuracy of the line frequency, which for most power installations is very good.

If now the primary coil of a spark coil is placed in the plate circuit of the thyratron, the output of the spark coil will be a series of evenly spaced sparks occurring at the rate of 60/sec. The complete circuit diagram for the spark timer as built by the authors is shown in Fig. 2, and the particular values of the elements are given in the caption. It should be noted in the circuit diagram that the transformer, in addition to serving as the filament supply for the 2050 tube, also serves in the phase-shifting circuit. The resistance R and condenser C are part of the phase-shifting circuit and while particular values for them are given any combination of R and C so chosen that the product RC equals 6×10^{-3} ohm farad will give the same phase shift as the values listed above. In using any particular condenser and resistor to make up such a combination it should be kept in mind that the values as marked on most resistors and condensers are only good to $\pm$ 10 percent. Thus it is best actually to measure their values and select a combination so that the product $RC=6\times10^{-3}$ is correct to within $\pm$ 2 percent. By changing this value of the product RC to some other value the thyratron can be made to fire earlier or later in the cycle if this is desired.

It is most important for the proper operation of this spark timer that the phase of the grid voltage with respect to the plate voltage be correct and this should be checked

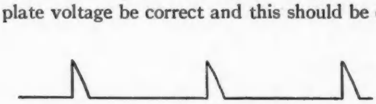

Fig. 3. Oscilloscope trace of voltage across plate resistor when grid voltage is properly phased.

Fig. 4. Oscilloscope trace of voltage across plate resistor when grid voltage is not properly phased.

before the circuit is operated or the thyratron will most likely be ruined. The check is made in the following manner. Replace resistor R_1 in the circuit with a resistance of about 200 ohms and connect the input leads of the vertical amplifier of a cathode-ray oscilloscope to the ends of this resistor. Then connect the circuit to a source of power, allow 30 sec for the filament of the thyratron to heat and then hold the tap switch down. The pattern on the oscilloscope should appear as indicated in Fig. 3. If the pattern appears as indicated in Fig. 4 it means that the phase of the grid voltage with respect to the phase of the plate voltage is 180 degrees from its proper value. To correct this it is only necessary to interchange the leads to the primary of the filament transformer. Doing this should result in an oscilloscope pattern as shown in Fig. 3.

Once the phase of the grid voltage is correct, remove the 200-ohm resistor and replace the 25-ohm resistor R_1 in the circuit. Starting with this resistor set at 25 ohms, gradually reduce its value until satisfactory sparks are obtained from the spark coil. However, do not reduce R_1 to a value less than 17 ohms as this will cause a serious overload of the thyratron.

[1] Paul E. Klopsteg, "Applications of spark recording to experiments in mechanics," *J. Opt. Soc. Amer.* and *Rev. Sci. Inst.* **19**, 335 (1929).
[2] See any standard text on vacuum tubes, e.g., Albert, *Fundamental electronics and vacuum tubes*, revised edition (Macmillan, 1947), p. 220; Fink, *Engineering electronics* (McGraw-Hill, 1938), p. 259; Reich, *Theory and application of electron tubes* (McGraw-Hill, 1939), p. 441.

AMERICAN JOURNAL OF PHYSICS VOLUME 18, NUMBER 6 SEPTEMBER, 1950

LETTERS TO THE EDITOR

Elasticity of Glass

THE experiment demonstrating the elasticity of glass and the incompressibility of water, described by Miller,[1] is a desirable variation of one to be found in the AAPT Manual, *Demonstration Experiments in Physics*.[2]

In the demonstration as Miller describes it, a geometrical principle is concerned, that for a given periphery of fixed length the circular shape encloses the largest area. The shape of bottle in his demonstration shows roughly an elliptical cross section. The capacity of the bottle is the product of this area, regarded as uniform from top to bottom, by the height of the bottle. This capacity, which is also the volume of the liquid, is constant. Squeezing the flatter pair of opposite walls *reduces*, because of elastic "give," the area within the ellipse, making it move farther away from the circular shape, thus decreasing the area and raising the necessary height of the liquid. Squeezing the bottle at its edge walls, however, *increases* this area, so that the height of the liquid must drop.

This demonstration calls to mind a previous experiment with similar equipment I once saw, long before 1938. Where Miller uses, for indicating the elastic "give," the change in level of the water meniscus in a capillary tube projecting through the cork, this older "demonstration" used the degree of buoyancy of a Cartesian diver, as described in the AAPT Manual mentioned. The bottle, filled with water, was corked tightly. A small inverted pill vial, open at the mouth and containing a suitable amount of trapped air, floated inside. Pressure with the hands on the flatter walls of the larger bottle caused the "diver" to descend, while release of this pressure caused its return to the top.

This demonstration was made especially dramatic, or melodramatic, and at any rate impressive, by inserting only part way through the cork, but apparently all the way through, a short piece of glass tubing to which was attached a length of laboratory rubber-hose. Performance of the experiment was in the presence of an uninitiated person, who observed the performer exhibit his "lung power" by "blowing" the floating vial to the bottom. The neophyte then took his turn at trying lung power. His uninformed manner of holding the bottle failed to give the considerable squeeze necessary for results. His blowing efforts were sure to be futile. It was a game of "blow the man down," or the man blowing himself down, rather than the diver.

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Los Angeles, California

[1] J. S. Miller, *Am. J. Physics* **18**, 164 (1950).
[2] Richard M. Sutton, *Demonstration Experiments in Physics* (McGraw-Hill, 1938), p. 124, M-321.

Is Frequency More Fundamental than Wavelength?

MOST books on general physics state that the velocity of electromagnetic waves changes when the wave front crosses a boundary between two media. This statement is followed by one which states in substance that there is a consequent change in the wavelength. No reason is given for this. Further, it is evident to the elementary student that the fundamental equation for a study of wave motion shows that a change in velocity can cause a change in either the frequency or the wavelength or both. This equation is usually written as

$$v = \nu\lambda,$$

where v is the velocity of the waves, λ is the wavelength, and ν is the frequency.

There seem to be several good reasons why the frequency must remain constant while the wavelength suffers a change. First of all, electromagnetic energy which crosses

a boundary between two media must cross in packets, each having energy

$$E=h\nu,$$

where h is Planck's constant. This means that if the energy of the quantum is to be the same inside the two media, the frequency of the electromagnetic radiation must be constant.

A similar argument will show that the wavelength is the logical quantity to change with velocity. This comes from consideration of the equation

$$mv=h/\lambda,$$

where m is the mass of the particle. This is the de Broglie wave equation which holds for small particles and for photons. As the momentum mv changes, which can be effected by a change in velocity, the wavelength must change. These are only two of the reasons which seem favorable to a change in wavelength rather than frequency when the velocity of electromagnetic waves is changed. Therefore, frequency seems to be the fundamental quantity rather than wavelength.

This conclusion implies that the frequency of light be listed as the quantity which determines color rather than the wavelength. This would give color the same significance in the study of light that pitch has in the study of sound. The wavelength listed in most elementary physics texts is the wavelength in vacuum. The wavelength of the light incident upon the retina is different from that usually listed because the velocity is not the same in the vitreous humor as it is in vacuum. To list the wavelength in vacuum as a determining property of certain colors is to mislead the student into the belief that the wavelength is the same for all media. On the other hand, the frequency is the same for different media and would definitely determine the color perceived independent of the refractive index of the vitreous humor.

MOODY L. COFFMAN

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Errors in the History of Science

I WAS pleased to read J. S. Miller's letter "Concerning historical references in general physics" in the February issue of the *American Journal of Physics*.[1] Dr. Miller's comment that more historical material should be included in a general physics course is a laudable one; yet there are certain drawbacks to this plan, as well.

The vast majority of textbooks using historical material do so in a rather naive and credulous manner. A physicist, who will usually be rather careful about the correctness of the physics material to be included in his book, will also usually completely neglect to check the historical statements included. Mythical tales of scientific hagiography are repeated without qualms. Let me cite an example, one, which to me is a good indication of how well the author has digested his material, and how much care he has taken in assembling his sources. This is the rather famous fable of Galileo's Leaning Tower of Pisa experiment.

The quotations cited below are all from recent textbooks in physics. Great care has been taken by the authors in each case to bring the book up-to-date by using the latest information available. Yet the following statements appear in the books.

> Moreover, all bodies fall with the same acceleration, regardless of their masses, as was first shown experimentally at the Leaning Tower of Pisa by Galileo Galilei (1564–1642), the Italian philosopher and astronomer.[2]
>
> Galileo demonstrated—according to some accounts by dropping objects from the leaning tower of Pisa —. . . .[3]
>
> . . . As tests he is known to have dropped various kinds of objects from different levels of the leaning tower of Pisa. . . . On one occasion, Galileo is alleged to have attracted a large crowd to the leaning tower. . . .[4]
>
> His [Galileo's] observations consisted of finding the time required for bodies of various sorts to fall to the ground from the leaning tower of Pisa.[5]

Now the story that Galileo did perform this leaning tower experiment was disproved in a very careful study by Lane Cooper.[6] A parallel and independent study was carried out by A. Koyré,[7] who also reached the conclusion that the story was a myth. Yet these careful studies are ignored by the writers of physics textbooks, with very few exceptions.[8]

Of course the fault is not strictly the physicist's. A good number of so-called histories of science, or physics, repeat the very same fables, even adding imaginary illustrations of the Leaning Tower experiment.[9] Just two examples, picked at random, will show this point:

> Galileo carried two iron blocks of different weights to the top of the Leaning Tower of Pisa . . . and dropped them at the same moment. . . . The reverberations of those two blocks crashing to earth together have not died down yet . . . for Galileo thereby broke the Aristotelian shackles. . . .[9]
>
> The famous Leaning Tower . . . at Pisa probably provided the suitable height and platform from which to drop the masses, and before the assembled university professors and students Galileo allowed a heavy shot and a light shot to fall together.[10]

Of course, unreliable secondary sources such as these make it rather difficult for a layman to be exact in his use of historical material. Much has been written in the history of science, quantitatively. Qualitatively, not very much has been done as yet. There is only now emerging a scientific view in the history of science.

I believe that more harm than good can be done by the incorrect use of history of science. It is, for example, harmful to implant in the student's mind the idea that Aristotelian science was overthrown by the dropping of cannon balls off a tower. It took centuries of conscious mental labor to establish the newer view in science. If Galileo has to be used (as he should be), then it would be more important to point out his experiments with the pendulum

and the inclined plane; it would be important to point out that his greatness lay in his skillful blending of mathematics and observational science, that he was the first mathematical physicist in the modern sense of the word.

The question arises how the layman (and, after all, the physicist, no matter how competent in his own field, is still a layman as far as the rather involved, and highly technical field of the history of physics is concerned) can choose his sources. The answer to this is not easy, for there does not exist a carefully annotated bibliography of the history of physics. (I am currently working on such a bibliography, a long and tedious job, with the results only dimly in sight.) Until a good guide to the sources exists, history of physics might better be kept out of general physics textbooks (unless the author can get a competent historian of science to either help him, or read the manuscript before publication). Or it might be safest to merely cite a man's published work, without refering to anecdotes (although not all anecdotes are folklore).

J. S. Miller's conclusion should be amended: There is much to be said for the *correct* historical . . . in a first physics course.

VICTOR STEINHART

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[1] Julius Sumner Miller, *Am. J. Physics* **18**, 115–116 (1950).
[2] Hausmann and Slack, *Physics*, ed. 3 (Van Nostrand, 1949), p. 57.
[3] Margenau, Watson, and Montgomery, *Physics principles and applications* (New York, 1949), p. 67.
[4] White, *Modern college physics* (New York, 1948), p. 42.
[5] Howe, *Introduction to physics* (New York, ed. 2, 1948), p. 43.
[6] Cooper, *Aristotle, Galileo and the tower of Pisa* (Ithaca, New York, 1935).
[7] A. Koyré, *Annales de l'Université de Paris* **12**, 441–453 (1937).
[8] Richtmyer and Kennard, *Introduction to modern physics*, ed. 4 (McGraw-Hill, 1947), p. 14—reference is made to Cooper's book.
[9] Fraser, *Half hours with great scientists: the story of physics* (Reinhold, 1948), p. 99. For a review of this naive, but otherwise good history see Guerlac, *Physics Today* **1**, 27–28 (December, 1948).
[10] Anthony, *Science and its background* (London, 1948), p. 142.

A Dynamical Demonstration of $f=R/2$ for a Concave Spherical Mirror

A RATHER striking and simple demonstration of the relation $f=R/2$ for a concave spherical mirror may be performed by employing a simple pendulum to measure the curvature. The mirror is placed face up on the lecture table as shown in Fig. 1. A simple pendulum consisting of a wooden ball and string is mounted directly over the center of the mirror. The correct adjustment and length are quickly obtained by shifting the string clamp at A and adjusting the length AC until the ball just contacts the mirror at all points when set into oscillation with an amplitude CE equal to one-half the mirror aperture. The projection lamp L mounted above the pendulum support and slightly off the vertical axis and adjusted to give an intense beam of parallel light is turned on and the room darkened. The adjustment should be such that the reflected beam of light converges to the point B on the pendulum string now at rest. Some chalk dust obtained by tapping two erasers together will make the light rays visible to a large audience. The string is pinched with the fingers of the left hand at the focal point B and holding this point fixed with the left hand the ball is swung in the arc CDA with the right hand to demonstrate that $BC=f=R/2$. A mirror of considerable aperture and reasonable focal length is desirable for this demonstration. The one used by the author has an aperture of 25 cm and a radius of curvature of approximately 50 cm.

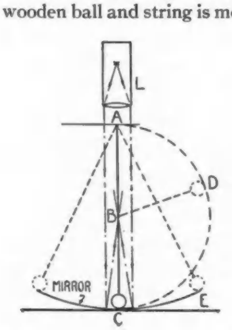

FIG. 1. Measuring the radius of curvature of a mirror with a simple pendulum.

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Demonstration of Beats and the Doppler Effect

THE phenomenon of beats is readily demonstrated by sounding two tuning forks of slightly different frequencies.[1] The Doppler effect is also easily demonstrated by swinging a vibrating 2000-cycle fork at arm's length in a wide horizontal arc, so that in part of the swing it advances upon the class and in the second half of the swing it recedes from the class.[2] The following demonstration couples these phenomena in an interesting way.

Establish a clearly audible beat frequency in two mounted resonant tuning forks by loading one of them. Let the class get the "feel" of the pulse frequency. Now, manually move the higher frequency fork rapidly toward the observers and rapidly away. This is easily done by thrusting it forward at arm's length and quickly drawing it back. The observers will report an increased beat frequency as the fork advances and a correspondingly diminished frequency as it recedes. A beat frequency of about 4/sec for the forks at rest appears to give optimum results.

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[1] Richard M. Sutton, *Demonstration Experiments in Physics* (McGraw-Hill, 1938), S-106.
[2] See reference 1, S-150.

Damped Electrical Oscillation Demonstrated with a Cathode-Ray Oscilloscope

A SIMPLE demonstration showing the form of a damped electrical oscillation can be performed using a Type 208 Dumont CRO. The interesting feature is that no auxiliary equipment is needed other than a tuned circuit with, perhaps, a variable resistance for varying the damping factor.

One side of a parallel circuit is connected to the ground terminal of the CRO, and the other side is connected to both the vertical input terminal and the external synchronization signal terminal. The synchronization signal switch is placed in the external position. In this position the tuned circuit is in parallel with the grid circuit of the 884, the sweep generator. At the beginning of the sweep the drop in the plate potential of the 884 appears in the grid circuit through the plate-to-grid capacitance. The sweep sawtooth is not affected by this change in the grid circuit, since the tube is not conducting during the sweep time. The only effect is that of electrically shocking the grid circuit into oscillation. This free oscillation appears across the vertical deflection plates.

The synchronization-signal-amplitude potentiometer must be at a setting other than zero and can be used to vary the initial impulse to the tuned circuit. The vertical deflection amplifier must be used since the oscillations have amplitudes of only a few volts.

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AMERICAN JOURNAL OF PHYSICS VOLUME 18, NUMBER 6 SEPTEMBER, 1950

ANNOUNCEMENTS AND NEWS

Arthur Jeffrey Dempster, 1886–1950

WITH the death of Professor Arthur Jeffrey Dempster on March 11, 1950, the University of Chicago lost one of its most eminent faculty members, and the Argonne National Laboratory an outstanding scientist. He was Professor of Physics in the Division of the Physical Sciences of the University and Director of the Argonne National Laboratory's Division of Mass Spectroscopy and Crystallography.

Dr. Dempster was born in Toronto, Ontario on August 14, 1886. He attended the University of Toronto and obtained the Master of Arts degree in 1910, after which he studied in Germany. He attended the University of Chicago from 1914 to 1916, obtaining his Ph.D. there in 1916. Following a period of service in the United States Army during World War I, he began a series of researches which demonstrated his mastery of experimental technique and his acumen in selecting important problems.

In 1913 J. J. Thomson, using a primitive form of the mass spectroscope, had discovered the existence of isotopes. It is small wonder that Dempster, beginning his career at the time, was attracted to this field. He brought with him a formidable array of talents, some of which may be analyzed. He had a scholarly interest in physics which led him to read widely and become well oriented in the subject; he had an unusual technical ability in the laboratory; his intelligence and foresight directed him toward important problems and thus he did not dissipate his time on scientific trivia. The impact of these qualities on the problems of mass spectroscopy was soon apparent. In the first place, he developed a source of positive ions such that positive ray beams could be formed from materials not obtainable in the gaseous form. Second, he devised a method of decreasing the exposure time required to register the analysis of the beam.

It was characteristic of Professor Dempster that beginning in 1935 he explored exhaustively most of the periodic system, thus making the first isotopic analyses of platinum, palladium, iridium, and gold. He discovered uranium 235 in 1935. In 1938, he published his most important paper in which he collected his results, together with those of others, and compiled a table and chart which essentially give the energy content of the nuclei of the known atomic species.

When the Metallurgical Project came to the University of Chicago, Professor Dempster's laboratory was incorporated in the effort. He was given financial assistance and experimental equipment such as had not been pre-

Arthur Jeffrey Dempster

viously available and his joy at the new opportunities, which were continued in the postwar Argonne National Laboratory, was obvious. He remained on leave of absence from the University until the time of his death, and worked every day in his laboratory.

In 1923, PROFESSOR DEMPSTER met Germaine Collette, a Belgian scholar who was studying in this country, and the friendship soon resulted in marriage. Mrs. Dempster kept up her scholastic interests and became a well-known authority on Chaucer. Her research in this field never failed to arouse the sympathetic interest and admiration of her husband.

PROFESSOR DEMPSTER was admired by his colleagues for his high standards of research and scholarship. He had a keen interest in University affairs and strong opinions concerning the role of the faculty in University administration. He was a member of the National Academy of Sciences and of the American Association of Physics Teachers; he served as President of the American Physical Society in 1944; and he received the Lewis Award of the American Philosophical Society. In 1937, the University of Toronto awarded him an honorary degree of D.Sc.

SAMUEL K. ALLISON

JOHN TORRENCE TATE

John Torrence Tate, 1889–1950

THE death of JOHN TORRENCE TATE on May 27, 1950, removed from the community of American physicists one of its most respected members and wisest counsellors. The work for which he is best known to the general scientific public is his Editorship of *The Physical Review*, since 1926. It has been largely through TATE's unremitting efforts that *The Physical Review* has grown to meet the rapidly increasing publication demands in the field of pure physics, with the maintenance of high editorial standards. He was also active in the establishment of the American Institute of Physics for the correlation of the activities of all of the major organizations of professional physicists in this country, and has served on its Governing Board since its inception. In 1939 he was President of the American Physical Society, and in 1942 was elected to the National Academy of Sciences.

TATE started his scientific career with the B.S. and M.A. degrees from the University of Nebraska in 1910 and 1912, respectively. His doctorate work was carried out at the University of Berlin, from which he received his degree in 1914. Here he came into contact with Professor James Franck from whom he gained his lifelong interest in the mechanism of electron-collision processes and the determination of the energy levels of atoms and molecules. It was in the making of precise measurements in this field that he himself, and his students with him, made his principal technical contribution to the advancement of physics.

After a brief return to the University of Nebraska, TATE moved to the University of Minnesota in 1916, where he became a full professor in 1919. In 1937 he was made Dean of the College of Science, Literature, and the Arts, but in 1943, during his extended leave on war work, he decided to resign this position and accept appointment as Research Professor of Physics. It was to this post that he returned at the close of the war, and which he retained until his death.

During World War I TATE served in the Signal Corps and Air Service. In World War II he acted as Chief of Division 6 of the National Defense Research Committee (NDRC), which was charged with the responsibility for research and development in undersea warfare. For this work he was awarded the Presidential Medal of Merit, with citation, by the U. S. Government, and the King's Medal by the British Government.

This brief review of Tate's activities and honors by no means completes the roll, but it is sufficient to give the measure of the man in the eyes of his associates in the world of science and affairs. To his many students, and to his friends, he will remain rather as the unassuming teacher, willing always to discuss a knotty problem, to give an encouraging word, or to get behind some worthy program of work. The spirit of clarity and rationality which pervaded his lectures on theoretical physics will not be forgotten soon by those fortunate enough to hear them, for many of whom they opened up new worlds of thought. *Quem di diligunt adolescens moritur.*

E. L. HILL

Book Reviews

Trilinear Chart of Nuclear Species. William H. Sullivan. John Wiley and Sons, Inc., New York, 1949. Price $2.50.

This compilation is remarkable in that an enormous amount of information on atomic nuclei has been condensed onto a horizontal strip $10\frac{1}{2}$ in. wide and about 15 ft long. This condensation is achieved by a clever combination of colors and lines. The chart is a beehive of hexagons, each representing a known nucleus, and each colored pale green or lavender, to indicate whether the nucleus occurs naturally or is produced artificially. Isotopes (nuclei having the same number of protons), isobars (nuclei having the same number of nucleons), and isotones[1] (nuclei having the same number of neutrons) stand out prominently, for isobars occur along the vertical axis of the hexagon, and isotopes and isotones along the other two axes inclined at 60° with respect to the vertical. Stability against alpha- and beta-decay are shown by colors of the rectangular panels near the top of the hexagons. For example, nuclei stable against beta-decay are represented by black panels, whereas those which are known to undergo beta-decay are in white. Nuclear isomerism is indicated by means of white lines running through the hexagons.

In addition, the nuclear constants such as nuclear masses, spins, magnetic moments, half-lives, nuclear radiations and their branching ratios are given. The table is quite up-to-date, as of June, 1949. However, it was noted that the spin of O^{17} is not listed; it is possible that this was omitted because the nuclear spin value is not yet too certain.[2]

Those who are interested, actively or otherwise, in nuclear physics will no doubt find this bird's-eye view of nuclear constants quite valuable.

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[1] For a discussion of this and other terms introduced in the chart, the reader is referred to a letter by T. P. Kohman, *Am. J. Physics* **15**, 356 (1947).

[2] See W. Low and C. H. Townes, *Physical Rev.* **75**, 529 (1949).

How to Solve Problems in General Physics. John Harty and Alfred H. Weber. Pp. 112, 6×9 in, plastic ring binder. Educational Publishers, Inc., St. Louis, Missouri. Price $2.25.

This book—covering mechanics, hydrostatics, heat, and thermodynamics—has been prepared as an aid to students in the first semester of college physics. Each of the nine chapters is introduced by a short discussion of theory after which several examples are solved in detail. In almost all the solutions a short discussion precedes any calculation, and in several a few words regarding the significance of the answers are included. English (engineering), metric, and mks systems of measurement are used, the last, however, somewhat sparingly. The units are carried through all calculations. Many of the worked examples require only a single step although in a number several answers are required. The illustrations are all line drawings of good quality. Except for a few items inherent in the use of a typewriter for mathematical symbols, little criticism is offered of the text itself. No doubt a discussion of fluid dynamics, elasticity, and wave motion is to be included in a second volume (light and electricity), which is reported to be in preparation. This problem book should prove to be satisfactory when used by students having limited time available for study. For other students, however, this book may be sufficiently attractive to offer the textbook considerable competition.

G. P. Brewington
Lawrence Institute of Technology

Elementary Pile Theory. Harry Soodak and Edward C. Campbell. Pp. 71, Figs. 21. John Wiley and Sons, Inc., New York, 1950. Price $2.50.

This book is based on a series of lectures given by Professor Soodak in the fall of 1946 as part of the training program of the Oak Ridge National Laboratory. The lectures were intended for students who knew nothing about pile theory, to give them a general view of the subject and to provide a background for later lectures of a much more technical nature.

In subject matter the book covers slowing down of fast neutrons (with and without capture) in an infinite medium and in one of finite extent, thermal neutron diffusion, steady-state and time-dependent pile equations, neutron "group" theory, reflector theory, and several other topics. The mathematical treatment is not difficult; Professor Soodak is blessed with the gift of presenting physical ideas with very little mathematical obfuscation. Many equations are written in parallel form, first in mathematical symbolism, followed by a verbal interpretation of each term of the equation, in a way which should be most helpful to the beginner. The figures are well chosen, always with the aim of making the physical situation clear.

Turning to specific points in the presentation, the discussion of the neutron cycle in sufficient generality to allow an immediate extension to the case of nonthermal piles is very well handled. It should deepen the student's understanding of the more conventional treatment. The distinction between "material buckling" and "geometrical buckling" is carefully and consistently made, and it was the consensus of the training school students that this added materially to their ease of comprehension. Professors Soodak and Campbell are to be commended for their success in keeping the emphasis always on the physical processes involved, and for covering a complex field concisely yet without superficiality.

H. C. Schweinler
Massachusetts Institute of Technology

The Meaning of Relativity. Third edition. Albert Einstein. Pp. 150, Figs. 4, $5\frac{1}{2}$×8 in. Princeton University Press, Princeton, New Jersey, 1950. Price $2.50.

In the present, the third, edition of *The Meaning of Relativity*, Professor Einstein has added Appendix II entitled "Generalized Theory of Gravitation" to the material contained in the second edition of the book and has made no other changes. This review will discuss only the new material presented in this edition.

The author has written Appendix II in the same beautifully simple and lucid style used in the earlier portions of the book. He has succeeded in stating his problem clearly and stresses the ideas used in solving it. The development is straightforward but of necessity uses a somewhat sophisticated mathematical formalism, the tensor analysis associated with a nonsymmetric affine connection and a general second-order tensor.

Einstein believes that there exists a ". . . total field comprising the entire physical reality" and the "Generalized Theory of Gravitation" is concerned with finding a mathematical description of this field, the field structure, and then finding the laws, that is, the field equations, which determine the field.

He presents mathematical arguments for saying the field is described by a general second-order tensor g_{ik} whose symmetric part $g_{\underline{ik}}$ presumably describes the gravitational field. The antisymmetric part $g_{\underset{\vee}{ik}}$ then describes the remaining aspects of the total field, one of which would be the electromagnetic field. A physical system which has only gravitational and electromagnetic fields present would then be described by the general g_{ik} in which the general relativity gravitational potentials and Maxwell electromagnetic field strengths are combined. Since the symmetric and antisymmetric parts of tensors are transformed into themselves by coordinate transformations the separation of "potentials" and "field strengths" is always maintained.

This is a mathematical objection to considering the total field as described by a general second-order tensor. However, Einstein seems to feel that this objection is more than overcome by the mathematical freedom one gains in formalism for raising and lowering indices with a general g_{ik}.

A general affine connection Γ^k_{ij} is introduced and a definition of parallel displacement in terms of this is given. The Γ^k_{ij} and the g_{ik} are assumed to be related by the equation

$$g_{\underset{+-}{ik};l} \equiv g_{ik,l} - g_{sk}\Gamma^s_{il} - g_{is}\Gamma^s_{lk} = 0,$$

where the comma denotes the ordinary derivative. The choice of this particular equation from the various types of covariant derivations of the g_{ik} is governed by the general "Hermitian" principle:

All conditions for the field shall remain unchanged if the g *and* Γ [g_{ik} *and* Γ^l_{ik}] *are simultaneously replaced by their transposed* ($\tilde{g}$ *and* $\tilde{\Gamma}$) [g_{ki} *and* Γ^l_{ki}].

One of the mathematically difficult problems of the theory is to solve this equation for the Γ^k_{ij} as functions of the g_{ik} and their derivatives. In spite of this difficulty, the existence of this equation seems to have convinced Einstein that "the introduction of nonsymmetrical fields is indeed a natural procedure."

The field equations are derived from a variational principle in which the Lagrangian function is a function of the g_{ij} and Γ^k_{ij} each of which is varied independently of the other and the variations are taken to vanish at the boundary. The actual Lagrangian function chosen by Einstein is not as in general relativity

$$wg^{ik}R_{ik},$$

where R_{ik} is the contracted curvature tensor formed from the Γ^k_{ij} and w is the square root of the negative of the determinant of the g_{ik}, but is

$$wg^{ik}U_{ik},$$

where

$$U_{ik} = R_{ik} - \tfrac{1}{3}[\Gamma_{i,k} - \Gamma_{k,i} + \Gamma_i\Gamma_k]$$

and

$$\Gamma_i = \Gamma^s_{\underset{\vee}{is}}.$$

Further restrictions are imposed and the resulting system of field equations is

$$\begin{aligned} R_{ik} &= 0 \\ (wg^{\underset{+-}{ik}})_{;l} &= 0 \\ \Gamma_i &= 0 \\ w_{;l} &= 0. \end{aligned}$$

It should be noticed that the second of these (in view of the last) is equivalent to Hermitean relation discussed above between the Γ^k_{ij} and the g_{ij}.

The solution of these field equations is an extremely difficult mathematical problem and no one has "yet found a practical way to confront the results of the theory with experimental evidence." Nevertheless Einstein remarks, "The results developed in the preceding pages appear to me the natural extension of the general theory of relativity."

Einstein's generalized theory of gravitation as presented in this book is not complete. As he has pointed out in a recent article[1] the answer to the following purely mathematical problem is not yet known: Is the manifold of solutions for the above field equations as extensive as must be required for a physical theory?

In addition there is no discussion of the rules for interpreting the mathematical formalism in terms of measurements on a physical system. The theory deals with the total field only. Particles are presumably described by "lumps" in the field and their equations of motion are to be deduced from the field equations. However there is no discussion of how this total field and its "lumps" are to be related to the "physical" fields and particles that the experimentalist generates and "measures."

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[1] A. Einstein, "On the generalized theory of gravitation," *Scientific American* **182**, No. 4, 13–17 (April, 1950).

Experimenting with the power of the average speaking voice, the Bell Laboratories have figured it would take 500 people talking continuously for a year to produce enough energy to heat a cup of tea.—*News Lines*, published by the Michigan Bell Telephone Company, February, 1950.